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EFFECTS OF RESIDUAL TENSION ON OUTPUT AND ENERGY EXPENDITURE IN MUSCULAR WORK *

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The previous researches of Bills (1), Bills and Stauffacher (3), Courts (5), Freeman (15, 17), and Stauffacher (40) have demonstrated the facilitative and inhibitory effects of different amounts of simultaneously induced tension on both mental and muscular work.[1] Not only has it been demonstrated that there is an optimal facilitating amount of increased muscular tension for a given task (3, 5, 17, 40) but that, for muscular work at least, there is an optimal locus of the pattern of simultaneously induced tension for maximal facilitation (16).

The question is here raised whether or not *residual* tension produces similar effects on output in subsequent work with which it is coincident. In other words, does increased muscular tension, developed by some *previous* tension-producing task, have facilitative effects on a test performance similar to those produced by tension induced concurrently? Such a case should be expected if the facilitative effects of simultaneously induced tension are due to increased proprioception *per se* and not, as suggested by Block (4), to compensatory effort acting to overcome the so-called 'distraction' of the tension-producing task.

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[1] The increased tension has characteristically been induced by having the subjects grip a dynamometer, suspend weights, or push on spring levers while engaged in some unrelated type of work. Most of the research employing this general method has previously been summarized and reviewed elsewhere by Bills (2).

Freeman, in fact, has assumed in several of his writings (**17**, **18**, **20**) that residual and simultaneously induced tensions do have similar effects on performance. The assumption is entirely logical in view of the fact that there is no real dichotomy between residual tensions and those that are induced simultaneously.[2] And the validity of this assumption is rendered even more plausible by reason of the similar effects that are produced by simultaneous and successive stimulation in several other fields of research; *e.g.*, simultaneous and successive color contrast. Numerous other illustrations might be given where the products of residual and subsequently induced processes are similar to those of simultaneously induced processes.

This article reports a series of experiments designed to test the general hypothesis already suggested above: *viz.*, that muscular tensions, residual from the first of a double period of ergographic work, influence output in the second performance.

Our check of this hypothesis is to be compared with the pioneer work of Bills (**1**) on simultaneously induced tension effects, but important methodological differences are to be noted. First, instead of inducing the residual tension by dynamometer gripping at some time preceding a subsequent *unrelated* type of work (a method which would seem to be logically suggested by Bills' study), we have chosen to initiate the residual tensions by prior work on a task *identical* with the test performance. This method is suggested by Freeman's (**16**) results in a later and more definitive study showing that the optimal locus of the pattern of simultaneously induced tension is in the proximate vicinity of the primary reacting muscle group. Furthermore, we have chosen muscular work instead of mental work[3] for both tension-producing and test performances because of the methodological difficulties involved in isolating and measuring the focal pattern of residual tension following mental work (**21**). Although so-called mental work is well known to involve the musculature, its motor residua are probably not of sufficient intensity, duration, and stability to permit easy measurement by present day methods. For purposes of expediency, therefore, we have chosen a task which is not only identical to the test performance, but one for which the motor components of its residua are relatively pronounced, long lasting, and measurable.

[2] No distinction can be drawn between them except with reference to the temporal relations between the tension-producing and test performances.

[3] In our use of the terms 'muscular' and 'mental,' we do not wish to imply any fundamental dichotomy between the types of work thus designated. We subscribe to current motor theory, but the experiments of Jacobson and Max are too well known to justify reviewing the evidence here. By muscular work, we mean those performances in which the movements of the musculature are gross and immediately obvious; whereas, by mental work, we mean those performances in which the motor components can be detected only with the aid of sensitive recording apparatus.

Our check of the aforementioned hypothesis utilizes certain well-known phenomena in the field of ergographic work: *viz.*, (1) the decrement in output manifested in the second of double work periods when interpolated with too brief a rest pause, such decrement being directly though not proportionately related to the brevity of the rest pause; (2) the 'warming up' effect, or temporary increase in output not attributable to practice, which is manifested in second work periods following optimal intervals of rest; and (3) the failure of this warming up effect to be manifested upon task resumption following lengthier rest pauses.

Three experiments will be described. The first traces over a period of time, by means of action potential records, the actual course of residual tensions in the resting but previously reacting muscle group following a standard period of ergographic work with maximal effort. The second studies the relative output in double periods of such work when interpolated with different intervals of rest, such intervals having been chosen as critical on the basis of the results of the first experiment. The third experiment studies the relative general energy cost, measured in terms of log conductance increments, of equivalent first and second work periods optimally separated by fifteen minutes of rest. Finally, the psychophysiology of so-called 'rest pause' effects will be discussed.

Resumé of Rest Pause Effects in Muscular Work

It has long been known that work decrement is usually arrested, and that total output is generally increased, if continuous work is occasionally interspersed with brief rests. This phenomenon is usually discussed as a 'rest pause' effect, and numerous studies have been made for industry on the placement, frequency, manner of passing, and duration of rest pauses for maximal output in specific kinds of work.[4] One generalization that has been drawn (**26**) from these studies is that the maximal beneficial effects of rest pauses on total work output depend upon optimal relations between the aforementioned variables.

The chief criticism of the work on total rest pause effects in industry, from a systematic standpoint, is that the subjects do not exert maximal or uniform effort when setting themselves for a protracted series of work periods. Consequently, one cannot say whether relative output in a work period following rest is primarily determined by work residua of previous effort or by less easily defined factors such as incentive, suggestion, self-instruction, attitude, relief from boredom, etc.

Occasional laboratory studies have been made, however, wherein relative output is studied in successive work periods of maximal effort separated by varying rest pauses. These afford a

[4] Excellent reviews of this work are contained in the writings of Graf (**26**) and Wyatt (**43**). The German *Arbeitspsychologie* group and the English *Industrial Fatigue Research Board* have been particularly active in this type of research.

somewhat better basis for speculation as to the course and nature of work residua and their relation to so-called rest pause effects. These studies may be placed, although somewhat arbitrarily, in two groups: (1) those in which *several* successive work periods are separated by a *standard* rest interval, and (2) those in which *double* work periods are separated by *varied* rest intervals.

In summarizing the main results of each group, we shall concentrate particularly on those studies in which muscular work was performed, although at times we shall introduce studies on so-called mental work in the same categories mentioned above. We shall be especially interested in the relative work output after various rest intervals for possible hints as to the course of residual tensions. That is, if a certain rest interval is found to be optimal for a given task, in terms of our hypothesis we should expect the reacting muscles not only to have regained their former tonicity, but to have gained a state of supernormal tension as well.

The experiments of Manzer (**31**), Yochelson (**44**), and Wells (**41**) are representative of the first group wherein relative work output is studied in several successive periods of maximal effort separated by standard intervals of rest. The results of most such studies show that total output is increased and that work decrement is arrested, although a 'warming up' effect is sometimes manifested in the early work periods before any decrement occurs. Yochelson found that, after a sharp initial decrement, output in moderately heavy ergographic work is fairly uniform for a relatively long time in succeeding work periods. In the earlier experiment by Wells on finger tapping, however, a progressive improvement, rather than a sharp initial decrement, was demonstrated in the earlier work periods; and Robinson and Heron (**35**) have pointed out that this temporary improvement is clearly greater than any day to day practice effect. Wells' study is now generally recognized as the first experimental demonstration of the 'warming up' effect. Matsui and Kabayashi (**32**) have more recently demonstrated the warming up effect, separable from practice, during the earlier of several successive work periods in finger tapping, strength of grip, and standing broad jump as well.

The second group of studies, wherein *double* work periods of maximal effort are interpolated with *varied* intervals of rest, is best typified by the experiments of Robinson and Heron (**35**) and certain others done by Manzer (**31**).

Robinson and Heron decisively confirmed Wells' previously mentioned findings by demonstrating that the warming up effect in a tapping performance is clearly separable from any practice effect. Furthermore, they showed that it is dependent upon the relative discontinuity of the work; *i.e.*, there is an optimal degree of discontinuity (as determined by the length of the rest pause) which maximally favors manifestation of the warming up effect, whereas decrements are directly related to the brevity of the shorter rest pauses. This, of course, reconciles the apparent lack of agreement in the results of the first group of studies reviewed above.

Robinson and Heron performed another experiment on the backward recitation of the alphabet and again obtained the warming up effect, the appearance of which also varied with the discontinuity of the work. Thus, the warming up effect is not unique to muscular work alone, but characteristic of mental work as well. Heron (**28**) later demonstrated warming up effects in another type of mental work (learning nonsense syllables) which were also quite distinct from any practice effects.

Matsui and Kabayashi (**32**) also varied length of rest pauses and, while they used several successive rather than double work periods, warming up effects separable from practice appeared. Specifically, they found that, in strength of grip, finger tapping, and standing broad jump, the warming up effect appeared sooner with the longest of the rather limited series of rest intervals employed.

While Manzer (**31**) was not primarily interested in arranging conditions to study warming up effects, the factors responsible for them apparently operated in the second of his double work periods which were prolonged to the point of 'exhaustion.' Manzer studied the effects of five, ten, and twenty minute rest pauses on relative percent of recovery in five ergographic tasks, each employing different muscles. For the entire group of subjects, average percent of manifested recovery in each task increased with the length of the rest pause, and, even though the average curve of manifested recovery was negatively accelerated, it showed some promise of continuing to rise had longer rest pauses been introduced. Complete or practically complete recovery was manifested after the longest rest pause in four of the five tasks, and in one of these 'recovery' was slightly greater than complete (by four percent; warming up effect?).

For many of Manzer's subjects, percent of manifested recovery after a five minute rest was significantly greater than after a longer pause of ten or twenty minutes. For example, in the task involving the finger flexor muscles, average percent of manifested recovery for two-thirds

of the subjects was sixteen percent greater after a five-minute rest than after a longer one of ten minutes; and, for the group as a whole, average 'recovery' was also greater (by two percent) after the shorter interval. No warming up effects were evidenced in any of these performances, but it is nevertheless significant that manifested recovery was greater after the shortest of the two shorter rest intervals, even though it was greatest after the longest rest. These facts lead one to suspect that Manzer's subjects were laboring under the preconception that recovery is directly related to length of the rest pause, and this may have had a pronounced effect on the relative amount of effort expended after the various rest intervals. Evidently the work residua of previous effort after five minutes rest maximally favored manifestation of the warming up effect in this particular task, but the subjects did not take maximal advantage of their facilitating effects.

A study by Graf (**27**) can perhaps be classed here even though it reports changes in attitude of the worker toward task resumption during a prolonged rest rather than subsequent output. Graf reports that the worker's willingness to resume work undergoes a series of changes. Immediately following heavy work, the attitude toward task resumption is very unfavorable; then there is a progressively increasing willingness toward resumption after a brief rest, which in turn is followed by a reverse in trend. Graf's study is significant in providing another possible index as to the course of the work residua which presumptively are of primary importance in producing so-called rest pause effects.

Postulated Relations between Residual Tensions and Rest Pause Effects

The studies reviewed in the foregoing section suggest that, if residual tensions are a determining factor in so-called rest pause effects, their course during a prolonged rest is not that of a gradually fading 'trace' but is cyclical in character. Since output varies with length of rest interval, then the reduced output in second work periods following too short a rest pause should be correlated with a state of decreased tension in the reacting muscles. This is apparently supported by the empirical fact that muscles become relatively flaccid shortly after cessation of strenuous exercise. Similarly, if our postulated relation holds, then the so-called warming up effect, manifested in second work periods following optimal intervals of rest, should be correlated with a state of supernormal tension in the reacting muscles at resumption of work. The failure of the warming up effect to be manifested after prolonged intervals of rest should be correlated, in terms of our hypothesis, with a loss of supernormal tension in the reacting muscles.

None of the studies previously cited bear directly upon the course of residual tensions and their relations to subsequent work output. But they do indicate a method for the investigation of our postulated relations, *viz.*, the double work period of maximal effort; and they also show that an optimal rest period is necessary for the maximal warming up effect to become manifest.

Our immediate problem, therefore, can now be more clearly defined. In order to check our postulated relationships between residual tension and subsequent work output, we should first trace the course of residual tensions in the resting but previously reacting

muscle group following a standard period of ergographic work with maximal effort; then arrange double periods of such work separated by different rest intervals, one of which permits task resumption to coincide approximately with the expected state of maximal residual tension.

Experiment I

Problem.—To trace, during a prolonged rest period, the course of residual tensions induced in the primary reacting muscle group by a standard period of maximal effort in moderately heavy ergographic work.

Apparatus and Procedure.—Each of a group of four male subjects was seated in a comfortable Morris chair and allowed to relax for about ten minutes. He was then given the signal to exert maximal effort at weight-pulling with the right arm while remaining in the same general posture. Following this work, he remained at rest (but alert) for thirty minutes, during which the course of residual tensions in the previously reacting arm was recorded.

Measurement of residual tensions took the form of action potential records, since these phenomena can be recorded from a relatively restricted muscular area. Small copper (2 cm.) electrodes were placed upon the right biceps muscle and maintained in fixed position and at a standard separation by an elastic arm band to which they were attached. These electrodes connected with a four-stage, resistance-coupled amplifier and an *electrical integrator* (**29**). Use of the integrator permitted us to read directly from a dial the total electrical disturbance (in microvolts) developed each minute between the electrodes.[5] The subject and the recording equipment were placed within an electrically shielded room, and measures of activity in the resting right biceps were taken both before and after the two-minute work period.

The subjects were trained in relaxation by previous practice in the experimental posture and work situation, and they were instructed to inhibit tendencies to voluntary and involuntary movement of any bodily part during the pre- and post-work periods. This procedure was introduced as a precaution against possible 'spread effects' from muscular activities other than those induced by the ergographic work.[6] As a further control over the muscle groups involved in the work, the weight-pulling apparatus[7] was so designed as to delimit the activity primarily to the right biceps. No records were taken of the actual amount of work done.

Results.—While several pre- and post-work action potential records were obtained from each subject, the detailed course of indicated residual tensions is quite individualistic; *i.e.*, maximal and minimal residual tensions are not manifested during the same minute fractions of the post-work period for all subjects, due possibly to differences in amount of work previously done, strength and general physical condition of the subjects, and other uncontrolled factors. However, there is a marked similarity in the individual records indi-

[5] The dial actually records the number of times a condensor discharges a standard amount of electrical energy developed by the system. This charge is a function of both the amplitude and frequency of action potentials by which Davis (**13**) and others quantify general trends in oscillograms from cathode-ray records. Because the integrator gives an immediate and automatic summation of total electrical effects, it is more feasible for protracted study of residual tensions than the usual types of recorders.

[6] The fact that work and resting postures were nearly identical also served as a further control.

[7] A complete description of the weight-pulling apparatus is given in connection with Experiment II. As to essentials that need be mentioned here, the apparatus limited the extent of movement so that rate of movement was the only variable.

cating the same general course of residual tensions; *viz.*, a rather sharp initial drop to a level approximating that of the pre-work period, then a gradual rise to a much higher level, which in turn is followed by a gradual decline to approximately the pre-work level. We have chosen to present only a characteristic record of each subject rather than an average curve. In Fig. I the total microvoltage developed by the biceps muscle for each of thirty successive minute periods during rest is plotted for each of the four subjects.

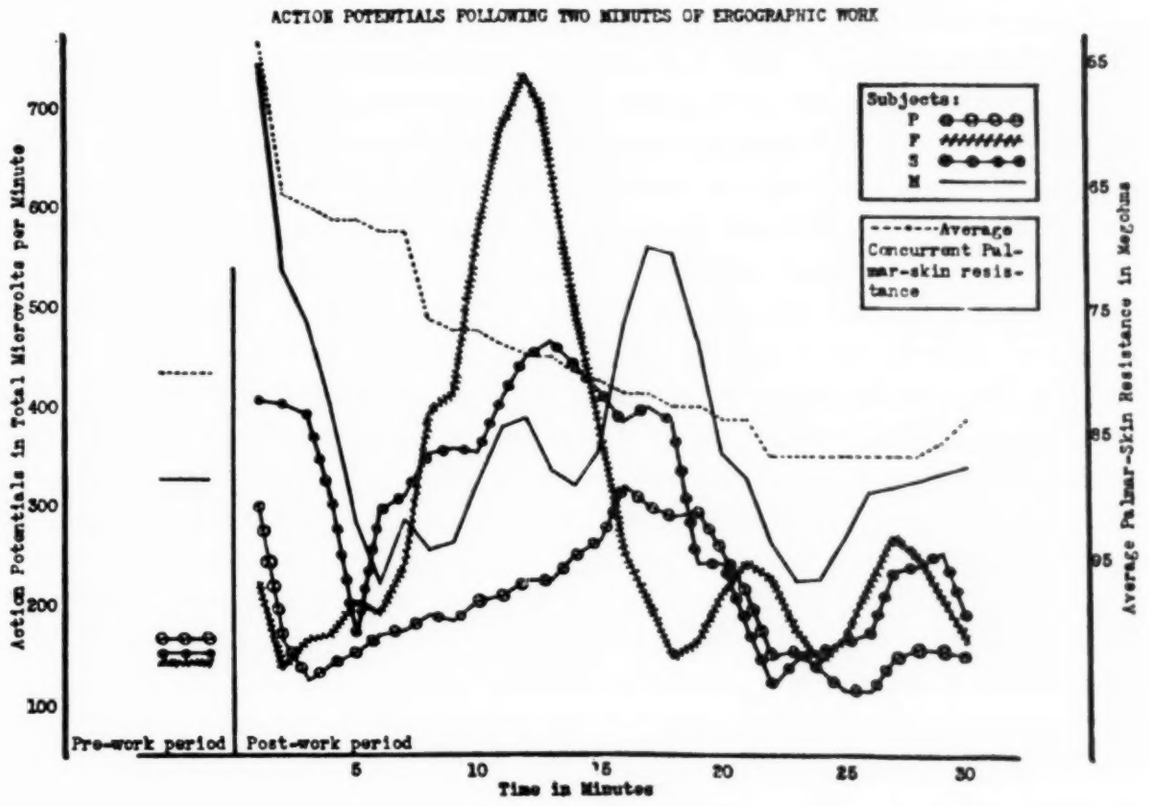

FIG. I.

The general course of residual tensions induced by the ergographic work is immediately apparent by inspection of the curves in the figure. Contrary to traditional conceptions of 'traces,' these residua are not of a gradually fading character throughout their entire course, but are actually cyclical, with a period of maximal activity around fifteen minutes after previous work.

The question arises as to why the recorded tensions immediately after work do not drop to a point far below the initial resting level as was hypothetically predicted. But one has only to realize that the action potential record from the previously reacting arm is not exclusively a function of excitation of local origin to understand why the records show greater activity during the first few minutes of rest than later. In any protracted ergographic work with maximal effort,

more and more of the general musculature becomes involved in an attempt to reinforce the activity of the primary reacting member. This was true in our experiment wherein subjects were observed to grit their teeth, brace their feet, stiffen their backs, and clench their fists progressively more and more as they approached the end of the work period. Such non-focal tensions are well known to spread their excitation to the primary reacting member and to influence any physiological record obtained from it. Following cessation of work, the primary reacting muscle group would presumptively have a tendency to relax very rapidly to a point far below its pre-work level if it were relatively less influenced by excitation from residual tensions of the general or auxiliary musculature.[8] Obviously the primary and auxiliary muscle groups should not be expected to follow identical courses of relaxation and recovery following work.

That excitation from the general musculature ceased to influence the course of residual tensions in the primary reacting member relatively early in the post-work period is indicated by concomitant records of palmar-skin resistance [9] taken on some of the subjects. The dotted line in the upper part of Fig. I is an average curve of these readings, and shows that the general excitation background falls gradually after work and levels off to remain relatively stable during the remainder of the rest period. What we have recorded and plotted in Fig. I, therefore, is the course of residual tensions (in a previously reacting muscle) superposed upon a negatively accelerated curve of general relaxation. It should be noted that the supernormal phase of residual tension in the arm occurs in spite of the fact that the subjects were generally relaxed at that time, and therefore cannot be explained as due to the spread of excitation from tensions of the general musculature. That the course of residual tensions or work 'traces' continues to be cyclical, although gradually reaching lower absolute levels, is suggested by the slight upswings in the plotted action potential records near the end of the thirty-minute rest period.

Discussion.—It should be obvious that the maximal state of residual tension would not necessarily arise at fifteen minutes or thereabouts following any task. The absolute course of residual tensions during rest is probably a function of the task, its duration,

[8] This has more recently been clearly indicated by the extremely rapid loss of previous tension in the primary reacting muscle group following lighter and shorter work involving the general musculature to a much lesser degree. *Cf.* the course of residual tensions (as indicated by action potential records from the primary lifting muscle group) in Freeman and Sharp's (**24**) extension of the traditional lifted-weight experiment.

[9] The work of Darrow (**8, 10**), Darrow and Freeman (**12**), Freeman and Giffen (**22**), Freeman and Simpson (**25**), and Wenger and Irwin (**42**) has shown the suitability of palmar-skin resistance as an index of general muscular tension and excitation background. Records were obtained by use of zinc-sulphate electrodes and a modified form of the Behavior Research Resistance Box (**11, 25**).

amount of work done, and the strength and general physical condition of the subject. What our results do show is that, under the work conditions imposed by our experiment, work residua which are muscular in character persist over rather long periods of time. Furthermore, they show that the primary reacting muscle not only loses and later regains its former tonicity, but also attains a state of supernormal tension at or about fifteen minutes after work, which in turn is almost completely lost after a thirty-minute rest.

Thus, according to our hypothesis concerning the relation of residual tensions to so-called rest pause effects, we should expect a second and similar work period imposed fifteen minutes later to yield significantly higher output scores ('warming up' effect); and, similarly, we should expect little or no warming up effect to be manifested in a second work period imposed after thirty minutes rest. The next experiment was accordingly designed to check upon these postulated relationships.

Experiment II

Problem.—To test the hypothesis that, in double work periods, the warming up effect and its later absence are manifested after rest intervals which permit task resumption to coincide with the supernormal and normal tension states, respectively, of the primary reacting muscle group.

Apparatus and Procedure.—Ten male subjects engaged with 'maximal effort' in two two-minute periods of moderately heavy ergographic work identical to that of the first experiment. These work periods were interpolated with rest intervals of fifteen and thirty minutes of rest. Judging from the results of the first experiment, these rest periods permitted task resumption to coincide approximately with the supernormal and subsequent normal tension states, respectively, in the reacting arms of the majority of subjects.

All subjects performed once under each interval condition on successive days, and differences due to possible practice and fatigue effects were minimized by counterbalancing the experimental orders for the two halves of the group. A preliminary work period on the previous day served to further minimize practice effects as well as to familiarize the subjects with the work task and situation. During the rest intervals subjects remained quiet and were cautioned to refrain from any unnecessary activity, particularly exercise and massaging of their previously reacting arms. Knowledge of absolute and relative amounts of work output were withheld, as was also the purpose of the experiment.

Several measures were employed toward approximating the greatest effort possible on the part of the subjects, and 'maximal effort' is hereby defined as that obtained under these conditions. First, only male subjects were used, since young men in our culture commonly take more pride than girls in their physical strength and, under competitive conditions, are more apt to try to demonstrate their superiority in this respect. Secondly, competitive conditions were arranged by announcing in the class from which the subjects were recruited that their output scores and relative standings would be posted in the classroom following completion of the experiment. Third, subjects were emphatically instructed before their first work periods to begin work at a fairly rapid pace, one which they knew they would be unable to maintain, and to do their utmost to maintain it, making as many strokes during the time limit as possible. Furthermore, it was impressed upon them that they were not to deviate from this general work method from one trial to the next. Fourth, the experimenter announced the lapsed and unelapsed time in the same tone of voice at the end of each fifteen-second fraction of the work periods. This was done in order

to take maximal advantage of the tendency to exert progressively greater effort under competitive conditions as the time limit is approached. Lastly, an attempt was made to preclude the subjects from 'soldiering' on their first trials and 'saving' themselves for their second trials. This was done in the following manner. On the first day of the experiment, knowledge that a second work period would soon be imposed was withheld from the subjects until after completion of the first work period. On the second day, subjects were specifically instructed not to let their knowledge of the impending second work period influence their relative effort in the first work period.

The weight-pulling apparatus was essentially a spring ergograph operated through a pulley system by a sixty degree movement of the subject's right forearm which rested on the broad arm of the Morris chair in which he sat. The subject grasped and moved between stops a hand-grip attached to the pulley system, and his elbow and shoulder were stabilized by means of an adjustable elbow pocket and straps. The initial tension of the spring system (before beginning the up movement of each stroke) was 1535.4 gms. This was the amount of suspended weight required to stretch the spring system to the point where it was stabilized. A Veeder-Root counter was attached to the pulley system in such a way as to register only complete strokes. This feature was called to *S*'s attention before each work period, thus emphasizing the necessity of making a complete stroke each time in order for it to count in his output score.

Nothing is to be gained in this experiment by converting number of completed strokes in each work period into foot-pounds, kilograms, or some other measure of absolute work output. Under the conditions of this experiment, all such measures are directly related anyway. Our results will be expressed, therefore, in terms of the simplest and most convenient measure, *viz.*, number of strokes completed in each two-minute work period.

Results.—The relative work output (completed strokes) obtained in first and second work periods under the two interval conditions may be seen by reference to the summary of results in Table I.[10]

TABLE I

WORK OUTPUT IN FIRST AND SECOND OF DOUBLE WORK PERIODS UNDER INTERVAL CONDITIONS OF FIFTEEN AND THIRTY MINUTES REST

($d.f.$ = 9)

	15 Min. Rest		30 Min. Rest	
	1st Wk. Pd.	2d Wk. Pd.	1st Wk. Pd.	2d Wk. Pd.
Mean Strokes	312.4	345.1	307.3	318.3
$Mean_{diff}$	32.7		11.0	
t	3.91		1.27	
P	<.01		>.20	

There was a mean increment of 32.7 strokes in the second work period (over the first) under the fifteen-minute rest condition, as compared with a similar increment of only 11.0 strokes under the thirty-minute rest condition. Furthermore, using Fisher's (**14**) t test of significance, the latter increment is not statistically significant (t is 1.27 and P > .20), whereas that under the fifteen-minute rest

[10] The raw data from which this and subsequent tables are compiled, as well as the data plotted in Fig. I, may be examined by reference to the appendix of the original manuscript (**37**).

condition is very significant (t is 3.91 and $P < .01$).[11] This means that the possibilities are less than five in a thousand that such a large work increment following fifteen minutes rest could have manifested itself by 'chance.' Similarly, the possibilities are greater than one hundred in a thousand that the small increment following thirty minutes rest could have so arisen. In spite of their significantly greater output in the second work period under the fifteen-minute rest condition, several subjects voluntarily reported feeling less fatigued than after the first work period.

Although the two halves of the group performed under the two interval conditions in counter-balanced order, the data were also analyzed to estimate to what extent day-to-day practice effects, as well as effects resulting from differential knowledge on the two days concerning the impending second work periods, might have influenced the above differences otherwise assumed as due to differential tensions operating at the time of task resumption.

The mean output scores for the first work periods, irrespective of subsequent interval conditions, are 313.4 and 305.3 for the first and second days, respectively. The difference of 9.1 strokes is in the opposite direction expected from practice but falls far short of statistical significance (t is .62 and $P > .50$). Similarly, that no day-to-day practice effect was manifested between second work periods is shown by the mean output scores of 339.8 and 323.6 strokes for the first and second days, respectively, and this difference (16.2 strokes) in the opposite direction expected from practice also falls far short of statistical significance (t is 1.07 and $P > .30$). Thus no significant output differences were manifested between either first or second work periods, day-to-day practice effects possibly having been masked by other residua of previous performance. This is suggested by the fact that some of the subjects complained of slightly sore arms when they appeared for their trials on the second day.

We may next examine to what extent relative output in first and second work periods under the two interval conditions was influenced by differential knowledge on the two days that a second work period was to be imposed. We should expect a greater increment in output to have occurred in second work periods on the second day than on the first if the subjects 'saved' themselves in the first work periods on the second day and exerted greater effort in their second work periods. That this was not true is clearly shown by the greater mean increment of 25.4 strokes manifested in the second work periods on the first day (irrespective of antecedent interval conditions), as compared with the smaller mean increment of 18.3 strokes on the second day. Furthermore, this difference of 7.1 strokes also falls far short of statistical significance (t is .53 and $P > .60$). It is obvious, therefore, that the output differences (shown in Table I) between first and second work periods under the two interval conditions are as much (if not more) a result of the subjects' efforts on the first day as on the second when they were more sophisticated.

In view of the small and insignificant differences revealed by the foregoing 'practice-knowledge' analysis, it may now be more confidently assumed that counter-balancing effectively eliminated output differences between first and second work periods under the two interval conditions not otherwise attributable to residual tensions. We may conclude, therefore, that the improvement in the second work period ('warming up' effect) after fifteen minutes rest accords with the prediction based upon heightened and specifically localized residual tension.

Discussion.—It will be recalled that this experiment assumed the

[11] In Fisher's test, the value of P designates the probability that a difference (in *either* direction) as large as that obtained would arise in similar samples if the true difference were zero.

operation of a cyclical course of specific residual tension similar to that found in the first experiment. But even had we actually checked the tension here, it might be argued that the pronounced improvement in second work after fifteen minutes rest was due to a 'volitional' compensatory increase in general bodily activity rather than to the specific 'hyper-recovery' tension of the reacting arm. Previous experiments have shown that when a subject is 'distracted' (**33**) or 'fatigued' (**23**) he may compensate by exerting greater effort. Such effort is accompanied by increments in general energy expenditure which are often sufficiently great to produce better than normal performance. We have more or less assumed that the pronounced improvement after fifteen minutes rest is not primarily due to overcompensations in general effort acting to offset 'fatigue,' but is referable instead to the heightened 'recovery' tension of the reacting arm. The subjects' voluntary reports concerning their relative feelings of effort and fatigue in first and second work are in accord with this assumption, but a more objective check is in order.

The question may also be raised as to whether the pronounced output increment after fifteen minutes rest was due to greater total or widespread bodily tension (general 'warmed up' condition, heightened general 'excitation background,' etc.) at the time of task resumption. This question is also not answered directly by the results of this experiment. But, if we may assume that the subjects here were as generally relaxed at the fifteen-minute post-work period as the subjects in the similar situation of Experiment I, it appears that the improvement in second work is primarily dependent upon the heightened 'recovery' tension of the primary reacting member. This is suggested by the following facts. Referring to Fig. I, we see that the post-work skin-resistance index of total residual tension dropped steadily during the thirty-minute post-work period and did not show the cyclical increase at fifteen minutes characteristic of the specific tension in the previously reacting arm. We see also that total non-specific tension at the fifteen-minute post-work period is about the same as before commencement of work, thus indicating that the subjects were not generally 'warmed up' at that time. It would appear, therefore, that the pronounced improvement in second work of *this* experiment was not due to greater total or widespread muscular tension. Incidental findings in the next experiment offer substantial support to this conclusion.

The following extension of this experiment, however, was designed primarily as an attempt to answer the first of these two questions.

Experiment III

Problem.—What is the relative 'efficiency' of optimally spaced first and second work periods? In other words, is the pronounced output increment (warming-up effect) manifested in second work of Experiment II after a fifteen-minute rest due to compensatory increments in general energy expenditure (increased effort)?

Apparatus and Procedure.—Fifteen male subjects, all without experience in the previous experiments, performed on each of two successive days in two two-minute periods of ergographic work identical to that of Experiment II. These double work periods were interpolated with a fifteen-minute interval of rest (relaxing in the Morris chair) which, as in Experiment II, presumably permitted task resumption to coincide approximately with the maximal state of muscular 'recovery' tension in the reacting arms of the majority of subjects. A period of relaxation (ten to twelve minutes) preceded the first work period also. Maximal effort was encouraged by the same procedures described in Experiment II; and, similarly, work output was expressed in terms of the number of strokes completed in each work period. Knowledge of absolute and relative amounts of work output, as well as the purpose of the experiment, was withheld from the subjects until after the experiment was completed.

In order to survey the problem of relative energy expenditure, measures of palmar-skin resistance were taken immediately before and after both first and second work periods. Subjects remained as quiet and relaxed as possible during these measurements, thus minimizing differences between pre- and post-work measures occasioned by bodily activities other than those persisting from effort during the work period itself. The fact that resting and working postures were nearly identical (reclining in the Morris chair) served to further minimize such differences. Both pre- and post-work resistance measures were later converted into 'log conductance scores,'[12] these apparently providing the closest approximation to equivalent units of total psychophysiological activity (energy expenditure) at the moment of measurement (10). The relative energy expenditure in each work period was then expressed in terms of the log conductance increment at cessation of work. This use of post-work increments in log conductance as an index of relative energy expenditure in the interpolated work period was adopted for the following reasons: First, because of the obvious and well known fact that all measures of bodily change (oxygen metabolism, breathing rate, heart rate, etc.) during and immediately after work are directly related (36); secondly, because of the known correlation between palm conductance and more generally recognized measures of energy expenditure such as oxygen metabolism (22) and insensible weight loss (12); and lastly, because of the methodological difficulties in obtaining reliably representative measures of resistance *during* work.[13]

Results.—Table II shows the average output (strokes) and the average indices of relative energy expenditure (log conductance increment at cessation of work) in first and second work periods for both first and second days as well as for both days combined. The respective amounts, directions, and reliabilities of both output and energy expenditure differences are shown in the lower half of the table.

[12] Log_{10} micromho conductance. Following the practice of Darrow (10) and others, the logs of conductance were multiplied by 100 in order to yield more convenient values for computation and presentation.

[13] Since we did not have a photopolygraph available for continuous recording of resistance changes during actual work, our only alternative to the above procedure would have been to take the average of as many separate readings as possible during work. The two-minute work period would have permitted only three or four (at most) of such readings, and their average would not have been very typical.

TABLE II

MEAN OUTPUT (STROKES) AND ENERGY EXPENDITURE (LOG CONDUCTANCE INCREMENTS) IN OPTIMALLY SPACED DOUBLE PERIODS OF ERGOGRAPHIC WORK
($d.f.$ = 14)

	First Day		Second Day		Both Days Combined	
	1st Work	2nd Work	1st Work	2nd Work	1st Work	2nd Work
Mean Strokes	284.9	301.5	285.7	304.3	285.3	302.9
Mean Log Conductance:						
Before Work	150.1	151.2	144.2	145.2	147.1	148.2
After Work	177.7	175.3	178.3	175.4	178.0	175.4
Increment	27.6	24.1	34.1	30.2	30.9	27.2
	2nd Work − 1st		2nd Work − 1st		2nd Work − 1st	
$Mean_{diff.}$ (strokes)	+16.6		+18.6		+17.6	
t	2.49		3.45		4.46	
P	<.03		<.01		<.01	
$Mean_{diff.}$ (log cond. increments)	−3.5		−3.9		−3.7	
t	2.42		1.25		2.39	
P	<.05		<.25		<.05	

It can readily be seen, by inspection of the output scores, that more work was done in the second work periods. The mean output increment for second work (both days combined) is 17.6 strokes, and this difference is very significant (t is 4.46 and $P < .01$), thus confirming similar results in Experiment II.[14] Furthermore, this held for both days of the experiment [*i.e.*, it did not 'wear off' with practice]. In fact, the difference between first and second work output on the second day (18.6 strokes) is slightly greater than that on the first (16.6) and is more significant as well; t is 3.45 and $P < .01$ for the difference on the second day as compared with that on the first where t is 2.49 and $P < .03$.[15] That the above increments in second work output are not to be attributed to practice may readily be seen from the fact that no appreciable day-to-day changes in output were manifested in either first or second work periods.

If the output scores alone were considered, one might propose an explanation of the improvement in second work in terms of greater

[14] The reader possibly may note with some surprise that, although this experiment is identical (skin resistance measurements excepted) to the fifteen-minute rest condition of Experiment II, the output scores here appear to be significantly lower. The same is true of the relative improvement shown in the second work period. This should not be especially surprising, however, in view of the fact that small and different groups of subjects performed in the two experiments. Three extremely strong subjects (varsity athletes in fact) were largely responsible for the higher output scores of Experiment II.

[15] In samples of this size, the value of t demanded for a P of .01 is 2.98 (difference 'very significant'), and that demanded for a P of .05 is 2.15 (difference 'significant') (14).

total or widespread tension (general 'warmed up' state, etc.) at the time of task resumption. The pre-work conductance measures, however, provide indices of the relative amount of such tension at commencement of first and second work. Thus an empirical check of this type of explanation may be made. Judging from inspection of these pre-work conductance measures (see Table II), it seems that on neither day was work resumed in the second work period with appreciably greater total bodily tension than at the start of the first work period. The average log conductance measures at commencement of first and second work periods (both days combined) are almost identical (147.1 and 148.2, respectively). While this small difference of 1.1 conductance units falls short of (but approaches) statistical significance (t is 1.45 and $P > .15$), the similar differences on each of the two days would obviously be even less significant.[16] It appears, therefore, that the fifteen-minute rest period proved sufficient to permit the subjects to relax to about the same general level before task resumption as before first work. Thus the improvement in the second work period is apparently not to be attributed to greater total bodily tension or a general 'warmed up' condition at the time of task resumption.

We may now turn our attention to the *post*-work conductance-increment indices of relative energy expenditure, and therein apparently lies the answer to the crux of our problem here. We see at once that energy expenditure appears to have been less in second work periods. This is indicated by the fact that the average log conductance increment at cessation of second work (both days combined) is only 27.2 as compared with that of 30.9 following first work, and the apparently negligible difference of 3.7 conductance units turns out to be statistically significant (t is 2.39 and $P < .05$). Furthermore, the differences are in the same direction and of the same relative magnitude on both the first and second days (3.5 and 3.9, respectively).[17]

Here apparently is evidence that the 'warming up' effect with optimal separation of double work periods is not dependent upon

[16] For example, the values of t and P for the difference of 1.0 conductance units on the second day are only 1.55 and >.50, respectively.

[17] While the difference is slightly greater on the second day than that on the first, the first-day difference is statistically significant (t is 2.42 and $P < .05$) and that on the second day falls short of such significance (t is 1.25 and $P < .25$). This would suggest that the apparently greater 'efficiency' in the second of optimally spaced double work periods would 'wear off' if similar performances were extended over several successive days. In other words, the facilitative effects of optimally localized residual tensions would be masked by other residua of previous performance. This is apparently supported by the fact that several of the subjects complained of slightly sore arms when they appeared for their trials on the second day; and such complaints were louder and more numerous when the subjects were encountered on the day following completion of the experiment. We shall attach special significance to these results in later interpreting the findings of Schubert (36) in a related experiment.

increments in general compensatory effort during second work. The results, in fact, are in the direction opposite to expectation from overcompensations for 'fatigue.' Efficiency also appears to have been greater (more output with smaller post-work conductance index of input) for second work performed after the fifteen-minute rest. This accords with voluntary statements of the subjects. As in Experiment II, several of the subjects were surprised at their increased output capacities and relative absence of effort in second work periods.

Discussion.—The conclusion apparently may be drawn that the improvement in the second of optimally spaced double work periods ('warming up' effect) does not depend upon either increased effort (overcompensations for 'fatigue') or greater total muscular tension, but is to be referred instead to the heightened 'recovery' tension of the primary reacting muscle. This is supported by the following facts. Experiment I showed that the skin-resistance index of *total* muscular tension following work did not show the cyclical increase at fifteen minutes characteristic of the action potential index of the *specific* tension in the primary reacting muscle. Furthermore, this index of total or widespread tension was about the same at the fifteen-minute post-work period as before commencement of work. *This* experiment showed that 'efficiency' after fifteen minutes rest from first work was apparently greater in the second work period (more output with smaller *post*-work conductance index of previous energy expenditure); and this occurred in spite of the fact that the *pre*-work conductance index of total muscular tension at the time of task resumption was not significantly different from that at commencement of first work.

This conclusion is given substantial support by Freeman and Sharp's (**24**) results in a more recent study on related 'rest pause' effects, *viz.*, the time-error function in lifted-weight judgments. They extended the traditional lifted-weight experiment by use of the action potential technique and *varied* time intervals between successive weight-liftings. As per traditional practice, the weight ('work output') in such successive weight-lifts (double 'work periods') was not only relatively constant, but the duration of such work was very brief. Furthermore, the weight-lifting movement was practically identical to that required for the work here. It should be obvious that the lighter and briefer work in the first 'work periods' of their experiment would not only result in relatively smaller increments in total widespread tension during the first weight-lifting, but that the residua of such tension coinciding with second weight-lifting would be relatively slight. Their results are as predictable from the results of this study. Specifically, they showed that the action potential index of the course of residual tension in the primary lifting muscle

is not only cyclical, but that it parallels the time-error index of relative effort in first and second weight-lifts. In other words, a preponderance of 'lighter' judgments were given for second weight-lifts coinciding with the period of supernormal residual tension following first weight-lifts (negative time-error or 'warming up' effect); whereas 'heavier' judgments resulted for second lifts coinciding with the period of decreased tension (positive time-error or 'fatigue').[18]

The results of these two studies on *successively* induced tension effects apparently provide the answer to Block's (**4**) criticism of experiments on *simultaneously* induced tension effects. She has pointed out that such experiments may have been contaminated by 'volitional' factors due to the 'distraction' of the tension-producing task. And if such is the case, the apparent facilitative effects of simultaneously induced tension might therefore be better attributed to overcompensations in effort rather than to the experimentally induced tension itself. This study on *successively* induced tension effects (as well as that by Freeman and Sharp) appears to be invulnerable to both the 'distraction' and 'compensatory effort' arguments. Whether the facilitative effects of residual tension are due to increased proprioception *per se*, however, will be reserved for later discussion.

That the facilitative effects of optimally focalized residual tension is not a unique phenomenon is indicated by reports of other researchers. Simonson and his co-workers (**38, 39**) have found that efficiency in lifting weights increases with periods of work up to an optimal length (depending upon the power delivery), while for longer periods it steadily decreases. We interpret this as indicating the importance of periods of sufficient length in order to develop an optimal pattern of supporting tension. Schubert (**36**), studying energy cost measurements on the curve of ergographic work, apparently found a similar trend in the work performed during the first few days of his experiment.[19]

No one has studied concurrently the relative tension in the focal and non-focal musculature during first and second of double work periods, but that appropriate shifts in tension pattern do occur in

[18] The above interchange of the traditional terminology used in experiments in ergography and lifted-weight judgments is intended to aid (rather than confuse) the reader in grasping the now-obvious relation between these hitherto independent fields of research.

[19] In an informal report of his first results to Robinson (**34**), Schubert states that "during work, the metabolism rises rapidly from the resting value to reach a peak by the fourth minute, then to *fall off slightly to a low point* at the eighth minute, only to rise slightly but steadily thereafter." In his formal report of his complete results (**36**) he states: "The cost-ratio of kilogram-meters output to c.c. oxygen used is *constant* throughout a twenty-two minute period of work at 140 kilograms per minute" (*italics* ours). We interpret the disappearance of the earlier trend as possibly due to the masking effect of residua other than optimally developed supporting tensions. The plausibility of this interpretation is suggested not only by our own results (see footnote 17), but by the fact that his experiment extended over several weeks with the use of only one subject.

protracted work conditions has been found by Freeman (19) and Daniel (7). Consequently, we are inclined to explain the greater efficiency of second work periods as due to more specific patterning or centering of the supporting tension. In other words, more work is done with less energy expenditure after an optimal period of rest because backlash proprioceptive excitation from optimally localized residual tension reinforces the voluntarily initiated contractions of the primary reacting member. Furthermore, optimally localized residual tensions apparently provide a well developed gradient through which the proprioceptive increments at the start and during second work are more directly channelized for expression through the primary reacting muscle group.

The Psychophysiology of 'Rest Pause' Effects

As shown in our historical resumé, output following a rest pause may be equal to, greater than, or less than that of the previous work period. All kinds of explanations have been advanced to account for these effects. They range from such ill-defined and ephemeral psychological concepts as 'fatigue,' 'boredom,' 'attitude,' 'incentive,' 'suggestion,' 'self-instruction' and 'aspiration level,' 'practice,' 'general orientation' and 'short-lived habits,' to the seemingly more substantial and ultimate physiological concepts of 'fatigue products,' 'muscular viscosity,' 'chemical re-equilibration,' 'warming up,' and 'refractory phase.' Most of the concepts in each group cannot be operationally defined through isolation of the phenomena which they are alleged to denote, and, as a consequence, their usage as explanations of rest pause effects is tautological and after the fact. For example, what insight comes from saying that when rest is followed by increased work the subject was 'warmed up' or that his reacting muscles were in a state of decreased 'viscosity'? One cannot predict the recurrence of similar rest pause effects upon the basis of such alleged causal factors because they have not been isolated and measured independently of their products. Furthermore, it seems reasonable to assume that all rest pause effects (decrement, increment, and no change) are interrelated in having a common set of determinants rather than such diverse and independent ones.

Our experiments have been guided by the principle of first determining the course of certain predictive signs of rest pause effects and later checking upon their relations to work output. We postulated that the course of residual tensions in the previously reacting muscle could be used as a basis for predicting the direction and relative magnitude of subsequent output changes. We set up a controlled work situation of two minutes duration and then recorded the actual

course of residual tension in the resting (but previously reacting) muscle during the next thirty minutes. This 'trace' was cyclical in character, with supernormal tension arising after approximately fifteen minutes, but later declining to the pre-work level at the end of thirty minutes. This suggested that, for work of the type imposed, supernormal performance would also follow a rest interval of fifteen minutes. Since it was already well known that too brief a rest pause is accompanied by decrement in subsequent work periods, we chose to compare output in second work periods following only fifteen and thirty minutes of rest. Results were as predicted upon the basis of specifically localized residual tension.

In obtaining these results, practice, fatigue, compensatory effort, and total or widespread tension (general 'warmed up' condition, etc.) were apparently effectively ruled out as possible explanations. Consequently, the rest pause effects here demonstrated are said to be due to those residual tensions which are related to the curve of recovery of the primary reacting muscle group.

While this suggests a more tangible and unitary basis for all rest pause effects, it does not reveal what more ultimate neural or biochemical phenomena are involved. For instance, is the curve of recovery of an intact muscle caused by a gradual absorption of fatigue products such as lactic acid from its surface, or is recovery largely an expression of the relative refractoriness of the nerves (and muscles) concerned in the previous performance? Work with isolated nerve-muscle preparations shows that, if the interval between stimulations (producing work) is too short, maximal recovery does not occur, whereas a more optimal spacing results in summational and *treppe* ('stair-case') phenomena. Lee (**30**) has shown that such phenomena can be produced without previous contraction by bathing the isolated muscle in a saline solution containing an appropriate amount of lactic acid. While the experiment apparently has never been done, it should be possible to study the relative amount of lactic acid present in a muscle at various intervals after work; and, if this course were found to be related to amplitude of response in the same way as our demonstrated relationship between residual tension and work output, a *local irritability* change might possibly suffice to explain rest pause effects.

Other considerations, however, must be taken into account in the intact organism where backlash proprioceptive excitation is also involved. The effect of such excitation in changing the threshold of intercalated spinal neurones, and even of the final motoneurones themselves, is well known (**6**). Immediately after cessation of prolonged work, a muscle tends to relax completely and there is thus a minimum of backlash excitation to the spinal segment. As the

muscle begins to regain its tonicity, later reaching a state of supernormal tension, more proprioceptive excitation is present in the spinal segment, and the primary motoneurone field of the muscle is then readier to respond to the voluntarily initiated contractions of the next work period.

Probably both local irritability and backlash proprioceptive excitation are involved in rest pause effects inasmuch as both are inseparably bound up with the recovery curve of muscular contraction. And while increased proprioceptive excitation might often be the result of increased local irritability, without the presence of its effects on the efferent part of the reaction arc, voluntarily induced contraction (as in muscular work) could not take advantage of the increased local irritability *per se*.

A final word needs to be said about a possible misinterpretation of residual tension effects. No necessary identity is implied between residual tensions and those of the previous work condition. A longitudinal survey will show only a functional relationship between the present tension condition and that of some antecedent period.

Summary and Conclusions

1. The course of residual tensions in the resting but previously reacting arm, following a two-minute period of maximal effort at moderately heavy ergographic work, is cyclical in character. A rather sharp initial loss in muscular tension is followed by a gradual rise to a much higher level at fifteen minutes than that of the pre-work period, then a subsequent slow decline to the pre-work level at thirty minutes.

2. Double work periods spaced so that the second coincides with the presumed state of supernormal 'recovery' tension in the reacting arm (15 min. rest period) results in significantly greater output over that of the first ('warming up' effect); whereas, when the second work period coincides with the presumed state of normal tension (no residual, 30 min. rest period), output is not significantly different from that in the first. These and previously known facts indicate that the course of residual tensions provides a unitary basis for predicting the direction and relative magnitude of so-called 'rest pause' effects in muscular work.

3. Optimally spaced double work periods show significantly greater output with significantly less energy expenditure in the second work period. This accords with the reports of subjects who were surprised at their increased efficiency and output capacities at a period when they might have expected to feel fatigued.

4. These results on successively induced tension effects support previous work on simultaneously induced tension effects. They

especially suggest that the latter cannot be attributed solely to volitional factors (*i.e.*, compensatory effort acting to offset a tension-producing 'distraction'), but that the facilitative effects of increased proprioception are probably also involved.

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References

1. Bills, A. G., The influence of muscular tension on the efficiency of mental work, *Amer. J. Psychol.*, 1927, **38**, 227–251.
2. Bills, A. G., Facilitation and inhibition in mental work, *Psychol. Bull.*, 1937, **34**, 286–309.
3. Bills, A. G., and Stauffacher, J. C., The influence of voluntarily induced tension on rational problem solving, *J. Psychol.*, 1937, **4**, 261–271.
4. Block, H., Influence of muscular exertion upon mental performance, *Arch. Psychol.*, N. Y., 1936, **23**, No. 202, pp. 49.
5. Courts, F. A., Relations between experimentally induced muscular tension and memorization, *J. exp. Psychol.*, 1939, **25**, 235–256.
6. Creed, R. S., Denny-Brown, D., Eccles, J. C., Liddell, E. G. T., and Sherrington, C. S., *Reflex activity of the spinal cord*, Oxford: Clarendon Press, 1932.
7. Daniel, R. S., The distribution of muscular action potentials during maze mearning, *J. exp. Psychol.*, 1939, **24**, 621–629.
8. Darrow, C. W., The galvanic skin reflex (sweating) and blood-pressure as preparatory and facilitative functions, *Psychol. Bull.*, 1936, **33**, 73–94.
9. Darrow, C. W., The significance of skin resistance in the light of its relation to the amount of perspiration, *J. gen. Psychol.*, 1934, **11**, 451–452.
10. Darrow, C. W., The equation of the galvanic skin reflex curve: I. The dynamics of reaction in relation to excitation-background, *J. gen. Psychol.*, 1937, **16**, 285–309.
11. Darrow, C. W., Uniform current for continuous, standard unit resistance records, *J. gen. Psychol.*, 1932, **6**, 471–473.
12. Darrow, C. W., and Freeman, G. L., Palmar skin-resistance changes contrasted with non-palmar changes, and rate of insensible weight loss, *J. exp. Psychol.*, 1934, **17**, 739–748.
13. Davis, R. C., The muscular tension reflex and two of its modifying conditions, *Univ. Ind. Publ., Sci. Ser.*, 1935, No. 3, 1–23.
14. Fisher, R. A., *Statistical methods for research workers*, Edinburgh: Oliver & Boyd, 1938.
15. Freeman, G. L., The facilitative and inhibitory effects of muscular tension upon performance, *Amer. J. Psychol.*, 1933, **45**, 17–52.
16. Freeman, G. L., The optimal locus of 'anticipatory tensions' in muscular work, *J. exp. Psychol.*, 1937, **21**, 554–564.
17. Freeman, G. L., The optimal muscular tensions for various performances, *Amer. J. Psychol.*, 1936, **51**, 146–150.
18. Freeman, G. L., The problem of set, *Amer. J. Psychol.*, 1939, **52**, 16–30.
19. Freeman, G. L., Changes in tension pattern and total energy expenditure during adaptation to 'distracting' stimuli, *Amer. J. Psychol.*, 1939, **52**, 354–360.
20. Freeman, G. L., The postural substrate, *Psychol. Rev.*, 1938, **45**, 324–334.
21. Freeman, G. L., The spread of neuromuscular activity during mental work, *J. gen. Psychol.*, 1931, **5**, 479–494.
22. Freeman, G. L., and Giffin, L. L., The measurement of general reactivity under basal conditions, *J. gen. Psychol.*, 1939, **21**, 63–72.
23. Freeman, G. L., and Lindley, S. B., Two neuro-muscular indices of mental fatigue, *J. exp. Psychol.*, 1931, **14**, 567–605.
24. Freeman, G. L., and Sharp, L. H., Muscular action potentials and the time-error function in lifted-weight judgments, *J. exp. Psychol.*, 1941, **29**, 23–36.
25. Freeman, G. L., and Simpson, R. M., The effect of experimentally induced muscular tension upon palmar skin resistance, *J. gen. Psychol.*, 1938, **18**, 319–326.
26. Graf, O., Die Arbeitspause in Theorie und Praxis, *Psychol. Arbeit.*, 1928, **9**, 563–683.
27. Graf, O., Ueber lohnendste Arbeitspausen bei geistiger Arbeit, *Kraepelin Psychol. Arbeit.*, 1925, **7**, 548.

28. HERON, W. T., The warming-up effect in learning nonsense syllables, *J. genet. Psychol.*, 1928, **35**, 219–227.
29. HOFFMAN, E. E., AND FREEMAN, G. L., An electrical integrator for 'action currents,' *Rev. Sci. Instr.*, 1940, **11**, 283–284.
30. LEE, R. S., The cause of the treppe, *Amer. J. Physiol.*, 1907, **18**, 267.
31. MANZER, C. W., An experimental investigation of rest pauses, *Arch. Psychol.*, N. Y., 1927, **14**, No. 90, pp. 84.
32. MATSUI, M., AND KOBAYASHI, T., The warming-up effect, *Jap. J. Psychol.*, 1935, **10**, 173–194.
33. MORGAN, J. J. B., Overcoming distractions, *Arch. Psychol.*, N. Y., 1916, **5**, No. 35, pp. 84.
34. ROBINSON, E. S., Work of the integrated organism, *In* Murchison, C., *A handbook of general experimental psychology*, Worcester: Clark Univ. Press, 1934, pp. 571–650.
35. ROBINSON, E. S., AND HERON, W. T., The warming-up effect, *J. exp. Psychol.*, 1924, **7**, 81–97.
36. SCHUBERT, H. J. P., Energy measurements on the curve of work, An analysis of the organic response in terms of oxygen metabolism, heart rate, and breathing rate, *Arch. Psychol.*, N. Y., 1932, **21**, No. 139, pp. 62.
37. SHARP, L. H., *Effects of residual tension on output and energy expenditure in muscular work*, Unpublished doctoral dissertation, Northwestern Univ., 1940.
38. SIMONSON, E., AND HEBESTREIT, H., Zum Verhalten des Wirkungsgrades bei koerperliche Arbeit, *Pflüg. Arch. ges. Physiol.*, 1930, **225**, 498–531.
39. SIMONSON, E., AND SIRKINA, G., Wirkungsgrad und Arbeitsmaximum, *Arbeitsphysiologie*, 1934, **7**, 457–474.
40. STAUFFACHER, J. C., The effect of induced muscular tension upon various phases of the learning process, *J. exp. Psychol.*, 1937, **21**, 26–46.
41. WELLS, F. L., Normal performance in the tapping test, *Amer. J. Psychol.*, 1908, **19**, 437–483.
42. WENGER, M. A., AND IRWIN, O. C., Fluctuations in skin resistance of infants and adults, and their relation to muscular processes, *Univ. Ia. Stud. Child Welf.*, 1936, **12**, No. 1, 141–179.
43. WYATT, S., Rest-pauses in industry, *Industr. Fatigue Res. Bd.*, Report No. 42, London: H. M. Stationary Office, 1927.
44. YOCHELSON, S., *Effect of rest pauses on work decrement*, Unpublished doctoral dissertation, Yale Univ., 1930.

MUSCULAR ACTION POTENTIALS AND THE TIME-ERROR FUNCTION IN LIFTED WEIGHT JUDGMENTS *

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Since the earliest days of experimental psychology it has been known that, in successive comparison judgments, the value assigned to the second member of a pair of stimulus objects is influenced by the factor of temporal separation. This time-error[1] was first noted by Fechner (1) in his work with lifted weights, and many elaborate routines have since been developed in an attempt to eliminate its effects in psychophysical judgments. More recently, the time-error has become a phenomenon of independent interest, especially for the light it may throw upon the manner in which 'traces' of previous performance influence subsequent response. Köhler (6) was the first to plot the *time-error function.* For comparison judgments of auditory intensities he showed that short stimulus separations (1–3 seconds) produce positive time-errors, while longer intervals (3–12 seconds) produce increasingly negative errors. We need not review here the numerous studies (9) following Köhler's. They show the influence of background stimulation, interpolated activity, absolute intensities of comparison stimuli, and practice upon the time-error function. We are primarily concerned with theories of the time-error function, and with the possible check of them by the action potential technique.

In the explanation of the time-error function, two major types of theory have been advanced. The first (attributed to Köhler) posits a physiological *trace* of previous sensory stimulation[2] which, after a slight initial rise (due to concentration of 'H ions' in the brain field), gradually falls away. The second (attributed to Woodworth (19)) extends the *set* theory of Müller and Schumann (8) by assuming that *S* makes his preparation for the second stimulus of a pair in terms

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[1] It is perhaps unnecessary to recall to the reader that an unfortunate terminology has classed as a *positive* time-error instances where the second of two equivalent stimuli is *underestimated*, and has called instances where the second stimulus is *overestimated* a *negative* time-error.

[2] The early memory image theory of Fechner receives but scant attention today due to the fact that comparison judgments can be made without reportable images of the first stimulus objects. Köhler's trace theory, however, may be thought of as a Fechnerian interpretation at a higher level of physiological sophistication.

of his set, or 'einstellung,' to the first, and that he uses the differential between 'set' effort and 'response' effort as the basis for the comparison judgment. These voluntary sets are by hypothesis more highly variable than the immediate physiological (reflexive) effects of first stimulation, but they too undergo a sinking with time to a point lower than the average set level, thereby accounting for the negative time-error.

Needham (**9**) has already pointed out that a single explanatory concept should suffice for both the positive time-errors of short intervals and the negative errors of longer intervals, and that either fading sensory trace or set adjustment should be adequate from this standpoint. However, there is growing evidence (**11**, **17**) that the time-error becomes increasingly less negative with prolonged intervals, and here both theories, in the present form at least, are inadequate.

Of course, both the trace of previous stimulation and the muscle's attunement for subsequent response will effect the condition of the brain field, where they meet excitation from the comparison stimulus. Stimulus and response cannot be divorced as far as their central (experiential) products are concerned, and the level of brain potential upon which the second stimulus-response sequence falls is a function of both traces of previous stimulation and backlash excitation of preparation for second lifting. Köhler, as a consequence of having studied comparison judgments, the motor components of which were not immediately obvious, has been led to ignore the possibility that there are some, and that these are inextricably bound up with the brain state in a total unitary process.[3] As a result he has failed to realize the feasibility of obtaining an objective check of his postulated brain states through their peripheral concomitants. On the other hand, set theorists have based their discussions of time-error functions principally upon lifted weight judgments, where motor adjustments under constant interval conditions are very marked. Furthermore, as a consequence of not having actually varied interval conditions for this type of comparison judgment,[4] the possibility has been overlooked that voluntary and reflexive motor reactions following the first lift contribute by way of backlash proprioception to the brain potential. In other words, the set theorist tends to center the judgmental process in the muscle rather than in the brain state to which muscle action contributes.

[3] This statement is an attempt to avoid a parallelistic notion of the relation of peripheral and central components of response. A somewhat similar concept was held by J. Dewey in his treatment of the reflex arc.

[4] This possibly occurs because of methodological difficulties in varying interval condition; that is, in lifted weight comparisons the stimuli cannot be presented mechanically but the subject, instead, has to present himself with them.

It is impossible to evaluate either the set or trace theory of the time-error as long as the only data are the judgments themselves. That is, the alleged causal factors of the phenomenon cannot be isolated from the products of their activity, and hence no prediction can be based upon either theory as now stated, except after the fact.

If we adopt instead the hypothesis that the brain state responsible for the comparison judgment is a product of the traces of first stimulation, reflexive after contractions, and proprioceptive excitation arising from preparation for the second stimulus, we may assume that the muscular tension changes occurring between periods of first and second weight-lifting constitute a symptom of the direction and magnitude of time errors which will be made at various intervals between lifts. The plausibility of this hypothesis is suggested by the following facts: (1) the cyclical tendency of the time-error function, (2) the cyclical character of 'rest-pause effects' in ergographic work (decrement, increment and no change, according as interval is varied), which also constitutes a type of time-error function, (3) the cyclical character of residual muscle tensions following a period of ergographic work, and (4) in double work periods separated by different rest intervals, the correlation between the course of residual tensions from first work and relative output in second work (**16**).

The purpose of our experiment can now be more explicitly stated: It constitutes a test of the hypothesis that the time-error function in lifted weight judgments parallels the changes in residual tensions in the muscle group previously engaged in lifting the first weight of the pair. The argument is developed in two experiments. Experiment 1 records the course of muscular action potentials for a period of 60 seconds following weight-lifting. Experiment 2 records judgments for weight pairs separated by intervals of 4, 8, 15, 30, and 60 seconds. The juxtaposing of these two curves throws new light upon the theory of time-error function.

Experimental Procedure

In the first experiment, the problem was to record the course of the muscular residua ('after-contractions') over a period of 60 seconds following weight-lifting. One subject, trained in progressive relaxation, lifted a weight of 500 grams through an arc of 60 degrees and returned it to the position of rest. The arm was arranged so that the lifting movement was confined largely to the biceps muscle, due to stabilization of the elbow and immobilization of the wrist. The entire lifting process was spaced so that one second was allowed for each 'up' and each 'down' movement. Prior to lifting the weight, *S* relaxed so as to provide a resting level of muscular action potentials. A ready signal preceded each lift, but inasmuch as a series of lifts were made within a short period of time, *S* was, in a sense, prepared for further lifting after the first ready signal. The interval between individual lifts was varied, however, to avoid building up an absolute time-expectancy for second lifting.

Muscular action potentials were recorded by use of small (2 cm) copper plate electrodes, attached over the right biceps by an elastic band, and connected with a four stage, resistance-coupled amplifier. The output of the amplifier fed into a sensitive A.C. voltmeter through a

blocking condensor, the entire system constituting an instrument similar to that described by Jacobson (5) for use in measuring the microvoltage of slight muscular contractions. The movement of the meter pointer was recorded photographically on a motion picture film which moved at a rate of 24 frames per second. Such records were later projected and the pointer position read for each individual frame. Since previous calibration had shown that each one-tenth volt division on the meter dial equalled five microvolts, the average microvoltage developed between the electrodes each second could be expressed.

In the second experiment, the problem was to obtain comparison judgments when weight pairs of the same relative magnitude as the stimulus in the first experiment were separated by various time intervals. The experimental series consisted of an equivalent number of stimulus pairs in which the second member was either heavier, lighter than, or equal to the first. The stimuli used were uniform sized cylinders which weighed 475, 500, and 525 grams.[5] They were discriminably different when lifted simultaneously, one with each hand.

Two subjects were used. They had previously been trained in making lifted weight judgments. They sat blindfolded in a Morris chair with the right arm resting comfortably on a broad padded board. The upright weight-cylinders were grasped around their sides rather than at the top, lifted 60 degrees to a padded stop, and returned to the board.

Instructions were as follows:

> Here are three different weights. The medium weight will be used as the standard and, at various intervals thereafter, I shall have you lift either the same, the lighter, or the heavier one. You are to judge the weight of the second member of the pair in terms of the first, as lighter, equal, or heavier. Attend strictly to the weight objects themselves, and lift at the time indicated by the experimenter. Lift weights from the elbow through the arc as you have been trained previously. Make your judgments immediately upon putting down the second weight. Always release weights immediately upon putting them down, but remain alert in the interval between comparison stimuli.

Since the time of weight presentation was very critical, a metronome set to click at second intervals was used throughout. The routine for each stimulus pair was as follows: counting with the metronome, *E* gave the signals for the first lift as "ready, lift, down," then permitted as many additional seconds to elapse as were necessary to complete the interval, then signalled "ready, lift, down" for the second weight of the pair. Fifteen seconds were allowed to elapse between the presentation of individual stimulus pairs.

A total of 105 judgments was obtained for each of five intervals, 4, 8, 15, 30, and 60 seconds.[6] In order to avoid intra-serial effects due to random variation of interval, *S* made 21 judgments at each interval before proceeding to the next; and to counterbalance the practice effect on different interval conditions, the following schedule was adopted for both subjects: 1st day, 4, 8, 15, 30, 60 sec.; second day, 8, 15, 30, 60, 4 sec.; third day, 15, 30, 60, 4, 8 sec.; fourth day, 30, 60, 4, 8, 15 sec.; fifth day, 60, 4, 8, 15, 30 sec. The same prearranged presentation order of weight pairs was used for all intervals. It included seven pairs in which the second stimulus was actually heavier than the first, seven in which it was lighter, and seven in which it was equal.

Results

Experiment I.—Action potential records from the biceps muscle showed a cyclical course following weight lifting. At the 'ready' signal, the potentials were increased markedly above the resting level, and the lift brought further increment. After the lift, there was an immediate relaxation of the muscle to a level below that of the 'ready' set, then an immediate rise in tension, and following this came a

[5] The use of weights so much heavier than the 100 grams of standard practice was adopted in order to induce greater activity in the biceps muscle and thus bring the action potentials into bolder relief.

[6] Upon the basis of action potential records of the first experiment, these intervals were chosen most likely to reveal significant differences in successive comparison judgments.

series of undulations in activity, the general trend of which was at first sharply downward and then gradually upward to a level approximating that of the previous 'ready' set. This general course, as indicated by average microvoltages developed at selected second intervals, is shown in Fig. 1. In this figure, *A* represents the resting

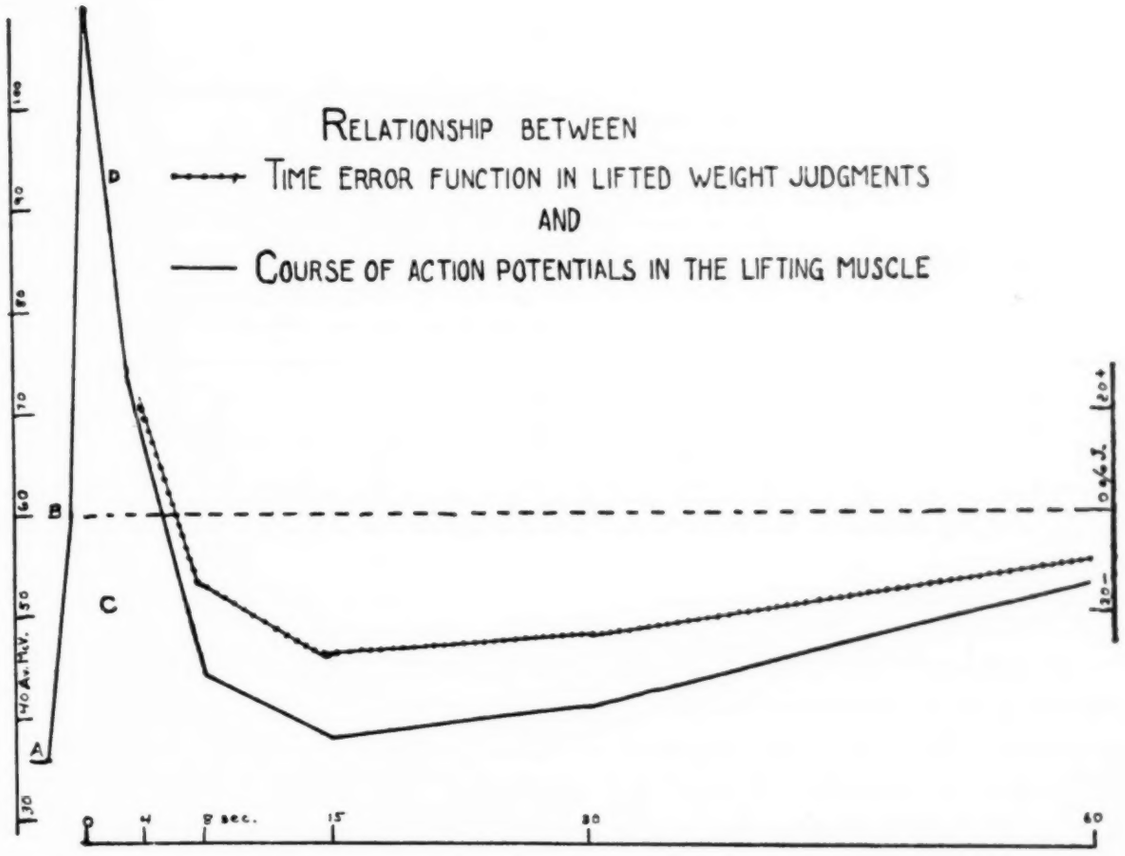

FIG. 1. Values marked along the ordinate at the left are average microvolts per sec., while those at the right are percentages of lighter (+) and heavier (−) judgments, respectively, greater than 'chance' expectancy.

level of muscular activity, *B* the 'ready' set level, *C* the immediate relaxation phase following the lift, and *D* the height of the 'after contraction.'[7] The other points on the curve represent the level of residual tension at 4, 8, 15, 30, and 60 seconds after the lifts.

The microvoltage values plotted in Fig. 1 represent averages for five individual lifts made by the same subject in a single experimental session. The individual data are given in Table I, from which it can be seen that, even though the course of residual tensions is cyclical in all cases, low and high points occur at different intervals. This is to be expected, since in lifting a given weight *S* will not always

[7] Question arises concerning the origin of this phenomenon. To some extent it may have been due to the slight rebound of the arm as it reached the board, and to the releasing movement. That after contractions are not due entirely to these factors, but rather are partly reflexive in nature, is indicated by the work of M. B. Sapirstein, R. C. Herman, and I. S. Wechsler (15).

mobilize exactly the same amount of tension, and the absolute course of subsequent muscular activity will be altered as a result. The set level, however, is relatively constant and in no case approximates the resting level of the muscle. Presumably this is due to the fact that *S* remained generally alert throughout the entire series, anticipating further lifts.

TABLE I

ACTION POTENTIALS DEVELOPED BY THE BICEPS MUSCLE AT VARIOUS INTERVALS AFTER LIFTING A 500 GRAM WEIGHT

Trial	Set Level	Seconds				
		4	8	15	30	60
		Average Microvoltage per Sec. at Various Intervals				
1	73	95	67	54	50	72
2	71	87	52	47	42	52
3	40	43	16	24	36	35
4	52	67	41	32	37	48
5	64	78	44	33	40	59
Average	60	74	45	38	41	53

Experiment II.—The difficulties of changing comparison stimuli prevents the determination of time-errors at intervals less than four seconds. In line with expectations from the results of the first experiment, a positive time-error appeared for both subjects at this interval, while at stimulus separations of 8, 15, 30, and 60 seconds the time-error was negative.

That the amount and direction of time-error are statistically significant is indicated in Tables II and III. In computing reliabilities for the time-errors, the following method was used. The raw data (see Table II) were first separated into lighter, equal, and heavier judgments. Equal judgments were discarded, in line with traditional practice. The time-error was then computed for each interval in terms of the percentage of lighter judgments in excess of 50 percent of heavier-plus-lighter judgments. For example, the 52 lighter judgments given for the four second interval by subject I constitute 27.61 percent greater (+) than the 50 percent of chance expectation. The standard error at each interval for an equal (50-50) percentage distribution of lighter and heavier judgments was then obtained by use of the formula $\frac{\cdot 5}{\sqrt{H+L}} \times 100$ (3). The critical ratios in Table II show that for subject I the time-errors for each interval except 60 sec. are highly significant; the same is true for

subject II except that the reliabilities for the 4 and 8 second interval conditions only approach significance.

The percentage of lighter judgments greater or less than 50 percent are averaged at each interval for the two subjects, in line with traditional practice, to give the time-error or *p*-function, plotted as

TABLE II

RELIABILITIES (*R*'s) OF INTRA-INTERVAL DIFFERENCES IN LIFTED-WEIGHT JUDGMENTS

Subject	I					II				
Interval Condition	4 sec.	8 sec.	15 sec.	30 sec.	60 sec.	4 sec.	8 sec.	15 sec.	30 sec.	60 sec.
Raw Data:										
No. Lighter Jdgmts...	52	23	22	24	31	49	28	16	14	26
No. Equal Jdgmts.....	38	26	18	28	30	26	30	35	34	39
No. Heavier Jdgmts...	15	56	65	53	44	30	47	54	57	40
	105	105	105	105	105	105	105	105	105	105
Intra-Interval R's:										
p (%) L Jdgmts. >50 $=100\left(\frac{L}{H+L}-.5\right)$	+27.61	−20.89	−24.71	−18.38	−8.67	+12.03	−12.67	−27.14	−30.28	−10.61
σ_{p-50}	6.11	5.63	5.36	5.70	5.77	5.63	5.77	5.98	5.93	6.15
C. R.	4.52	3.71	4.61	3.30	1.50	2.14	2.20	4.54	5.11	1.73

TABLE III

RELIABILITIES (*R*'s) OF INTER-INTERVAL DIFFERENCES IN LIFTED-WEIGHT JUDGMENTS

Subject	I						II					
Interval Comparison	4 and 8 Sec.		4 and 60 Sec.		15 and 60 Sec.		4 and 8 Sec.		4 and 60 Sec.		15 and 60 Sec.	
	4	8	4	60	15	60	4	8	4	60	15	60
Inter-Interval R's:												
p (%) L Jdgmnts. $=100\left(\frac{L}{H+L}\right)$	77.61	29.11	77.61	41.33	25.29	41.33	62.03	37.33	62.03	39.39	22.86	39.39
σ_p	5.09	4.14	5.09	5.69	4.66	5.69	5.46	5.59	5.46	6.01	5.02	6.01
p_{diff}	48.50		36.28		16.04		24.70		22.64		16.53	
$\sigma_{p_{diff}}$	6.56		7.63		7.35		7.81		8.12		7.83	
C. R.	7.39		4.75		2.18		3.16		2.79		2.11	

the upper (or crosshatched) line in Fig. 1. A close correspondence will be noted between the form of this curve and that indicating the course of residual tensions following a first lift. At the 15 and 30 second intervals the time-error is not as negative as the action potential curve would lead us to expect, however. This may be due to the tendency of the *Ss* to compensate for heavier judgments. That is, while it was not explicitly stated in the instructions, it would obviously have occurred to *S* that only one-third of the

stimulus pairs would actually contain a heavier second weight. Therefore, when he found himself giving an overwhelming preponderance of negative judgments, he would have a greater willingness to give lighter judgments in cases where the impressions were actually equal or his judgment doubtful. In spite of the fact that *S* was cautioned beforehand to judge on the basis of the immediate experience alone, compensation tendencies[8] probably functioned to flatten the time-error curve throughout.

The fact that there is such a close relation between the course of action potentials following first-weight lifting and the time-error function obtained in a similar situation offers substantial support to our original hypothesis. While we could have recorded action potentials during the actual process of making comparison judgments at different intervals of separation, the procedure would have required tremendous labor. Payne and Davis (**12**) have already taken such simultaneous records for a single interval condition of 3 seconds, indicating that lighter and heavier judgments can be predicted on the basis of concurrent muscular action potential records and that the amount of muscular effort exerted during the second lift is inversely related to the level of tension residual from the first lift.

In our experiments, the courses of both the residual tensions from first weight-lifting and the succeeding comparison judgments are cyclical in character. We have not determined reliabilities for the action potential curve, but the majority of points on the time-error curve are shown to represent true differences (see Table III). The 4 sec. values are significantly different from the 8 sec. values for both subjects, and the same would be true for the 4 to 15 sec. differences, inasmuch as these are even greater. Time-error differences between 4 and 60 sec. intervals are also highly significant, but those between 15 and 60 sec. intervals are not. Because the experiment stopped with a 60 sec. interval, neither curve returns completely to the expected baseline or 'set level.'

Discussion

Any theory of time-errors must be adequate to account for four groups of facts. These facts result from: (1) variations in the temporal separation of the stimuli compared, (2) variations of background stimulation, (3) variations of the absolute intensity levels of the stimuli compared, and (4) extended experimentation (practice). The results of our experiments bear directly upon only the factor of temporal separation, which we shall treat first. Later we shall ex-

[8] It is well recognized that in making a series of judgments the tendency for *S* to reverse his previous judgments becomes increasingly strong the greater the preponderance of one category of judgments forced upon him by the stimuli.

tend our interpretation of these results to the other aspects of the time-error function.

1. *Temporal Separation of The Stimuli Compared.*—The cyclical character of the time-error function has been suggested by previous work but has not been adequately demonstrated because experimenters in this field have confined their studies to a limited range of stimulus separations. Thus, Peak (**13**) found only positive time-errors when working with intervals up to .285 sec., while only negative time errors have been reported for most comparison judgments at intervals of 3 to 9 sec. Köhler's (**6**) study extended over 12 sec., showing a slight positive time-error at the shorter intervals, and then a negative time-error which became progressively larger with the longer time intervals. But Wada (**17**), by using intervals up to one minute, showed that the time-error becomes less negative and even positive for longer intervals. Most of the work on the time-error function has been done with auditory and visual stimuli, because of the greater ease in varying the interval. Heretofore, it has been only assumed that a similar time-error function holds in the field of lifted-weights as for other comparison judgments. Now, our experiments demonstrate that such is the case. The fact that our results are similar to those produced by comparisons of stimuli for which there are no pronounced muscular adjustments indicates the necessity of a unitary explanation of both sets of facts. Thus far, one set of data suggested the operation of sensory traces in the brain, and the other, both voluntary and involuntary adjustments of the musculature.

The fact that a so-called 'cognitive' or 'central' function of weight comparison judgment is inseparably bound up with changes in muscular tension does not warrant a strictly peripheral 'set' explanation. In the first place, residual tensions after lifting a weight are reflexive in character and not the result of a voluntary set made in anticipation of lifting a second weight after a definite interval. In the second place, while the activity level of a muscle will condition the amount of force exerted by it in a second lift, the central representation of this state is the primary condition for the comparison judgment. Set theory presumably would hold that *S*'s voluntary muscular preparation for second lifting varies with the interval. We do not deny that *S* makes voluntary set adjustments in preparation for lifting a similar second weight, but, since this preparation would be relatively constant for all intervals, it could not be the determining factor in the time-error function. Instead, the after-contraction of first lifting progressively changes the levels of background excitation in the brain field, and, as a result, the stimulus to second lifting, falling upon backgrounds of differing levels, in turn necessitates different degrees

of effort. The so-called 'voluntary' set for second lifting is obviously an integral part of the total peripheral-central comparison sequence.

On the other hand, it is also unwarranted to assume with conventional trace theory that motor reactions do not influence the brain state immediately responsible for comparison judgments but are merely reverberations and indifferent overflow phenomena. Our explanation of the time-error function in lifted-weight judgments recognizes both central and peripheral components in the total process. The amount of effort necessary to lift a weight is a function of the tension level of the lifting muscle at the beginning of the lift. As shown in our experiment, this level from which lifting increments arise undergoes a shift in time. Thus, if the second lift occurs when the muscular condition is higher than the set level of the first lift, a smaller increment in the tension of effort is produced by second lifting of the weight. Because the backlash proprioception from this act is less than that produced by first lifting (less effort), a smaller step-up in potential (excitation) occurs in the brain field, this presumably providing the basis for the phenomenal experience of lighter. But, if the second lift (at longer intervals) occurs when muscle tension has dropped to a level below that of the set for the first lift, a greater tension increment is produced by second lifting, and the corresponding step-up of potential in the brain field is much greater than that caused by first weight-lifting, thus producing the phenomenal experience of heavier.

We agree with Köhler that the immediate basis for comparison judgments is the relative step-up in brain potential produced by first and second stimuli of the comparison pair, and that *S* has no conscious awareness of the differential level from which these step-ups arise. But we would not agree with his implicit assumption that the brain field is divorced from the effects at the periphery immediately upon cessation of the first stimulus of a pair, so that changes in brain excitation (the 'silent image' or 'memory trace') are exclusively central and due to the dispersion and concentration of H-ions. For the exteroceptive stimulus does more than initiate brain activity. By setting up reflexive motor adjustments, differential backlash interoception is provided for modifying the strength of the exteroceptive trace in the brain field. In our opinion, a major factor by which a brain trace of previous stimulation becomes heightened or diminished in time is differential backlash excitation from the periphery. While this might be difficult to conceive for comparison judgments of light and sound, it is readily inferred in lifted-weight judgements upon the basis of our experiment. As we have shown, the residual tensions in the lifting muscle rise above the set level for the first lift, fall below that level, and then gradually return. By assuming that these

changes in peripheral activity are inextricably bound up with changes in central excitation, we may hypothesize what happens to the brain pattern by reference to the following figure.

In Fig. 2, the solid line represents the course of the level of excitation in the brain field after lifting the first weight of the stimulus pair. The change of excitation (potential step-up) produced by first weight lifting is indicated by the broken diagonal line at the extreme left, and the differential between first and second weight

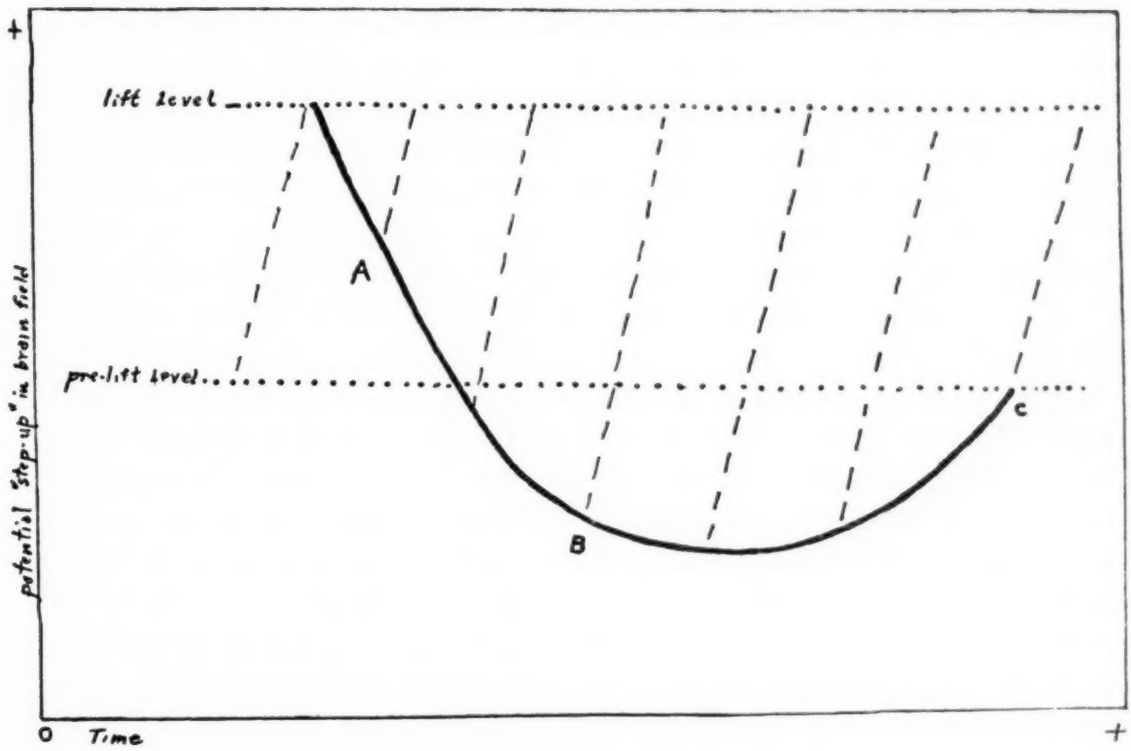

FIG. 2. Theoretical relationship of brain potential to time of comparison judgments; see text for explanation.

liftings at various time intervals can be seen by comparing the amount of step-up for the first lift with that for the second lift occurring at *A* (minus differential due to less effort in second weight lifting and hence a lighter judgment), *B* (plus differential due to necessarily greater effort in second weight lifting with a relatively relaxed muscle and hence a heavier judgment), and *C* (no differential due to equivalent effort in successive liftings and hence an equal judgment).

2. *Variations in Background Stimulation.*—In the previous paragraphs we have assumed a 'neutral' and unchanging *general* background of brain excitation upon which the focal stimulus effects were cast. Experiments by Lauenstein (7) have shown, however, that the time error function is modified by differences in background excitation. For example, visual comparison stimuli cast on a light background produced only positive time-errors which become progres-

sively greater with prolongation of the interval, while the same stimulus pairs cast upon a dark background produced the typical negative time-error function. In accounting for these reversal effects, Lauenstein speaks of the dynamic interaction or assimilation of the background stimulation with the focal trace. In terms of our diagram (Fig. 2), the facts might be interpreted as an increase in the absolute level of the brain trace throughout its entire course, under high intensity background excitation, and as an acceleration of the recovery process.

Traces of stimuli interpolated between the comparison pair provide another type of background excitation effect. As shown by Pratt (**14**) and by Guilford and Park (**4**), interpolated stimuli more or less intense than the standard stimulus have the effect of increasing or decreasing the negative time-error, respectively, when comparisons are made with a constant stimulus separation interval. In terms of our diagram, this would be indicated by changes in the relative step-up of the trace of first stimulation in reference to its corresponding 'set' level.

3. *Variations in the Absolute Intensity Levels of the Stimuli Compared.*—Use of comparison stimuli of a high intensity causes negative time-errors to appear at a given constant interval of separation, whereas use of weak stimuli causes the time-error to be positive, according to the work of Needham (**11**) on sound stimuli and the work of Woodrow (**18**) on lifted weights. Since in both of these studies the various intensities were presented in random order during the same experimental session, an excitation background or 'central tendency effect' representing the average level of individual traces at a given interval of stimulus separation would tend to be established. In terms of our hypothesis, the focal excitation produced by high or low intensity stimulation would interact with such background excitation to produce different absolute levels and courses of resulting traces. The trace of high intensity stimulation would fall to a lower level and recover more slowly than the trace of low intensity stimulation. Consequently, at the longer intervals of stimulus separation, the negative time-error would be much more pronounced for high than for low intensity stimulation.

4. *Variations Due to Extended Experimentation (Practice).*—The fact that the time-error function tends to reverse itself under extended experimentation [Needham (**10**)] is a challenge to any single type of explanation. Our hypothesis covers the facts no better than do previous trace or set theories, for the assertion that the course of residual tensions would reverse itself under practice provides no insight into the cause of such an alleged shift. Only one suggestion can be made at present as to the reason for the shift: the general

excitation level (representing the central-tendency effect of previous stimulation) becomes progressively higher, so that lifts which formerly functioned from a lower level of excitation (producing a preponderance of negative time-errors) now function from a higher level (producing a preponderance of positive time-errors). That is, when weight-lifts (or stimulus intensities) arise from a heightened excitation background, the effective stimulus is less intense and the step-up in brain potential is smaller than at the beginning of the experimental series. In Needham's experiment on practice effect, the change from negative to positive time-errors was gradual and resembled a learning curve. If this shift in comparison judgments could be attacked physiologically through the study of the central and peripheral aspects of general excitation background, we might be well on the way toward an understanding of even more basic phenomena of memory traces.

In proposing the above interpretations, we have obviously gone far beyond our experimental data. Our reason for doing so is a desire to integrate, in a tentative way at least, all the known facts which appear to be related to the time-error function, and also to suggest hypotheses which are capable of experimental check in greater degree than those heretofore submitted. We have checked our postulates only in connection with the factor of temporal separation. But, on the assumption that changes in the course of response residua are inextricably connected with changes in the brain field during comparison judgments, it seems possible to check upon the factors of general background excitation, absolute level of stimuli used, and practice. Furthermore, since the same phenomena appear to hold for lifted-weight judgments as for successive comparisons of light and sound stimuli, further experimentation into the physiological bases of time-error functions might well be confined to the first field, where their peripheral and central components can be thrown into bolder relief. The use of electroencephalography and muscular action potentials is indicated for these future studies, since either method alone will provide only inconclusive evidence on the interrelation of peripheral and central factors in comparison judgments.

Summary

1. The course of muscular action potentials following a first weight-lift of 500 grams is parallel to the time-error function in successive comparisons of similar lifted-weights.

2. Action potential and time-error curves are both cyclical in character. They show respectively: [*a*] increased potentials and a preponderance of 'lighter' judgments (positive time-error) when the

stimulus separation is short (4 sec.); [*b*] decreasing potentials and a progressive preponderance of 'heavier' judgments (negative time-error) when the interval is lengthened (8–30 sec., inclusive); [*c*] then a gradual increase of the action potentials toward the level which preceded first lifting, which in turn is paralleled by a gradual diminution of the negative time-error up to 60 sec.

3. An attempt is made to relate previous 'set' and 'trace' theories of time-error function by use of a modified set of postulates. Tentative application of these postulates is made to the known effects of stimulus separation, background stimulation, absolute level of stimuli compared, and practice, in order to suggest the further extension of the action potential technique in this field.

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References

1. Fechner, G. T., *Elemente der Psychophysik*, Leipzig, 1869.
2. Freeman, G. L., Concerning the field of field psychology, *Psychol. Rev.*, 1940, **47**, 416–424.
3. Guilford, J. P., *Psychometric methods*, New York: McGraw-Hill, 1936, 77–79.
4. Guilford, J. P., and Park, D. G., The effect of interpolated weights upon comparative judgments, *Amer. J. Psychol.*, 1931, **43**, 589–599.
5. Jacobson, E., The neurovoltmeter, *Amer. J. Psychol.*, 1939, **52**, 620–624.
6. Köhler, W., Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 1923, **4**, 115.
7. Lauenstein, L., Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Psychol. Forsch.*, 1932, **17**, 130–177.
8. Müller, G. E., and Schumann, E., Ueber die physiologische Grundlagen des Vergleichen gehobenen Gewichte, (*Pflueger's*) *Arch. ges. Physiol.*, 1889, **45**, 37.
9. Needham, J. G., The time-error in comparison judgments, *Psychol. Bull.*, 1934, **31**, 229–243.
10. Needham, J. G., The time-error as a function of continued experimentation, *Amer. J. Psychol.*, 1934, **46**, 558–567.
11. Needham, J. G., The effect of the time interval upon the time-error at different intensive levels, *J. exp. Psychol.*, 1935, **18**, 530–543.
12. Payne, B., and Davis, R. C., The role of muscular tension in the comparison of lifted weights, *J. exp. Psychol.*, 1940, **27**, 227–242.
13. Peak, H., Time order error in successive judgments and in reflexes: I. Inhibition of the judgment and the reflex, *J. exp. Psychol.*, 1939, **25**, 535–565.
14. Pratt, C. C., The time-error in psychophysical judgments, *Amer. J. Psychol.*, 1933, 45, 292–297.
15. Sapirstein, M. R., Herman, R. C., and Wechsler, I. S., Mechanism of after contraction, *Arch. Neurol. Psychiat.*, 1938, **40**, 300–312.
16. Sharp, L. H., Effects of residual tension on output and energy expenditure in muscular work, *J. exp. Psychol.*, 1941, **29**, 1–22.
17. Wada, Y., Der Zeitfehler, beim Sukzessivvergleich der Tonhöhe, *Jap. J. Psychol.*, 1932, **7**, 505–538.
18. Woodrow, H., Weight discrimination with a varying standard, *Amer. J. Psychol.*, 1933, **45**, 391–416.
19. Woodworth, R. S., *Experimental psychology*, New York: Henry Holt, 1938, 440–441.

VITAMIN A DEFICIENCY AND ITS RELATION TO HEARING [1]

BY MERLE LAWRENCE

Introduction

Incoördination and paralysis as the result of a vitamin A-free diet have been experimentally produced in dogs, rabbits, pigs, rats and chicks, and in several of these animals lesions of the central and peripheral nervous system have been observed. The most noticeable lesions consist of nerve demyelination in both afferent and efferent fibers, followed by complete disappearance of the fibers. The afferent fibers seem always to be the first influenced, and in the less severe cases they alone are changed.

One of the afferent nerves said to be most commonly affected by demyelination is the VIIIth. Incoördinated movements sometimes observed in the behavior of the animals are the result of degeneration of the vestibular branch of this nerve. Young dogs and rabbits become inattentive and inactive, and fail to move in response to noises or the voice. This lack of response to sound has been attributed to deafness, but it should be pointed out that it might result from postural deficiency or general apathy.

Mellanby (**7**) has reported the results, in dogs, of vitamin A deficiency upon the VIIIth nerve and the labyrinthine capsule in general. When the diet of the animals was kept normal [*i.e.*, contained a rich source of vitamin A], there were no pathological changes. When the diet was deficient, the extent of pathological change depended upon the length of time that the animals were deprived of the vitamin. All of the animals examined—dogs, rabbits, and rats—were reported to react in the same way to this deficiency. In addition to degeneration of different degrees in the cochlear nerve, there was also an accompanying disappearance of the cells of the spiral ganglia and their central and peripheral branches.

Mellanby (**5**) at one time suggested that nerve degeneration was the primary effect of a vitamin A-free diet. He also suggested that the other observable effects of the avitaminosis, such as changes in the various mucous membranes and changes of the skin and skin glands and other tissues of epiblastic origin, might be due to a loss of neurotrophic control. Furthermore, from his histological observations on dogs he thought it possible that nerve degeneration was caused by the pressure or by the stretching effect of a peculiar over-

[1] From the Psychological Laboratory of Princeton University.

growth of bone which was noted in the labyrinthine region. In the particular region of the labyrinth three separate areas of bony overgrowth were observed: in the modiolus of the cochlea, in the periosteal bone adjacent to the brain and internal auditory meatus, and occasionally in the basal whorl of the scala tympani.

From Mellanby, then, comes the view that nerve degeneration may result directly from the lack of sufficient vitamin A in the diet, or from the mechanical effects of an overgrowth of bone in certain regions adjacent to the nerves. On the other hand, Walbach and Bessey (**8**) have suggested that the skeleton may cease to grow while the nervous tissue continues, thus causing the nerves to be crowded until parts of the system degenerate.

Though the cause is not known, the VIIIth nerve in the dogs appears to have degenerated, sometimes completely, under the influence of the vitamin A-free diet. On the other hand, the evidence is not sufficiently clear to answer the question whether degeneration of the auditory nerve causes degenerative changes in the organ of Corti. With continued vitamin A deprivation in his dogs, Mellanby found an accompanying condition of serous labyrinthitis. This he believed to be the reason for the observed disappearance of the organ of Corti. In one animal put first on a deficient diet, there was found, on histological examination, no serous labyrinthitis and no destruction of the organ of Corti, though extreme nerve degeneration existed. The possibility suggested by the experimenter was that after the 15 weeks of vitamin A-free diet, nerve degeneration and serous labyrinthitis were present, but the latter had not been present long enough to affect the organ of Corti. The normal diet, rich in vitamin A, cleared up the infection, saving the sensory organ, but the nerve degeneration remained. Mellanby stated that what he observed in dogs also occurred in rabbits and rats.

The purpose of the present experiment was to discover in what way a vitamin A-free diet would affect the peripheral auditory mechanism of the rabbit. To test the extent of auditory impairment, the following observations were made.

1. Behavioral tests to check the hearing of the animal.
2. Cochlear responses for a study of inner ear functions.
3. Nerve potentials as recorded at the round window taken as an index to the extent of nerve degeneration, if any.
4. Histological studies made to determine the extent of impairment in the structures of the ear.

Experimental Conditions and Behavioral Observations

Fifteen rabbits of two breeds, Silver Martin and Dutch, were used in the experiment. At the age of six weeks, the age of weaning, they were separated into two groups, with breeds and litter mates divided as evenly as possible. Three of the animals died at the beginning of the

experiment, probably because they were weaned too early. During the course of the experiment four more animals died: one control, under an overdose of Dial anesthesia, and three experimentals, presumably because of the lack of vitamin A.

Of the eight remaining rabbits, five were controls, and three were experimental animals. Cochlear potentials were recorded in all of these animals, generally from both ears, making a total of fourteen ears examined. Nine of these were control and five were experimental ears.

The ears from all three experimental and those from one of the control animals were prepared for histological study.

The diet was one used by Mellanby (**6**) in an earlier study on the nervous lesions produced by a lack of vitamin A. According to the age of the rabbit, 40 to 70 grams of a 4-to-1 oats-bran mixture were given daily, together with 10 grams of alfalfa and a small amount of calcium carbonate. Lemon juice was given occasionally as a source of vitamin C. Alfalfa, which supplied the vitamin A, had to be treated for the experimental animals.

Only very small quantities of the vitamin are needed by the rabbit, so care is necessary to see that the experimental animal gets none. The presence of a very little will delay the effects of the deficiency for some time. To assure the destruction of the vitamin, the alfalfa was heated in an oven set at 120° C. for at least 36 hours. The food was powdered and spread on trays in thin layers. It was stirred now and then to assure better aeration and better oxidation of the vitamin.

The first observable effect of the avitaminosis was a gradually increasing opacity of the cornea. This sign began to appear about the second month of vitamin A-free diet. The xeropthalmia proceded until the animal was completely blind. The corneas erupted, the eyes dried up, and sometimes there was a bloody discharge from the rims, though this last effect might have been caused by licking on the part of the other rabbits. In addition to these symptons, the animals became generally inactive. They showed no interest in eating and lost weight rapidly.

When the animals were picked up they tried to escape by frantic kicking at the experimenter's hand. The only manifestation of muscular incoördination in the deficient animals was an inability to kick high or hard enough to reach the restraining hand. One extremely deficient animal, after three months of vitamin-free diet, when picked up went into a severe tremor, opening its mouth and uttering a cry. The fit started with an attempt to escape and ended in an inability to stand or move. In about twenty minutes the animal had recovered and ate a little. The same afternoon the operation was performed and electrical responses were taken.

According to the report by Mellanby, the animal should show signs of deafness under conditions of extreme avitaminosis. It seemed desirable to have some behavioral criterion by which the animal's hearing could be measured. This criterion was the response to a loud sound produced by a Galton whistle. The healthy animals always gave the response of pricking up the ear to this stimulation. The experimental animals, however, soon ceased to respond. Since it seemed that they might be too sick to respond, the experimenter jerked the cage every time the whistle sounded. The rabbits, no matter how sick, soon responded at the sound of the whistle, antici-

pating the jerk by giving a conditioned postural response. Two of the three animals which died from lack of the vitamin gave this response up to the evening before death; the third animal was too sick to stand.

The above behavioral tests were used to furnish a rough indication of the animal's hearing. The cochlear potentials recorded at the round window served as an index of the effect of the lack of vitamin on the auditory organ. Mellanby reported nerve degeneration. degeneration of the cells of the spiral ganglia, serous labyrinthitis and subsequent degeneration of the hair cells as a result of avitaminosis A. If the degeneration of the hair cells occurred, this fact should be indicated in the nature of the electrical response to tones.[2]

The experiments were performed under Urethane anesthesia. The source of tone was a beat frequency oscillator feeding into filters, attenuators, and a loud speaker connected to a tube which conveyed the sounds to the animal's ear. The stimulating apparatus was calibrated in dynes per square centimeter.

The cochlear potentials were picked up from the membrane of the round window, which was exposed by operation.

The region of bone lying most nearly over the round window lies between two groups of muscles and consists partly of the mastoid process of the mastoid bone. This process can be felt just below the base of the pinna where it joins the bony external auditory meatus. The jugular process of the occipital bone lies posterior and central to the mastoid process. It is covered by the occipital group of muscles which arise from the atlas and axis and are inserted on the occipital portion of the skull. At its region of origin, at the mastoid process, the sternomastoideus muscle runs parallel to this group of occipital muscles.

A slit in the skin about 2 cm long is made just over the palpated mastoid process and the underlying fascia carefully separated. Just behind the point of the mandible, which is slightly anterior to the operated region, the anterior facial vein joins the posterior facial vein to form the external jugular. If the opening is made too far forward the venous junction may be cut. Therefore great care must be taken in approaching the muscles lying in this region.

The occipital muscles are separated from the bands of the sternomastoideus and the latter removed from the mastoid process. The region of exposed tympanic bulla which lies between the mastoid and jugular processes is cleaned and drilled. If the drilling is done carefully the endostium, lining the inner wall of the bulla, will be exposed and may be opened by cauterizing. In enlarging the opening the mastoid process is removed, care being taken that the tympanic ring is not damaged. Those animals which had been subjected to a vitamin A-free diet showed an abnormal growth of bone around this region. This is the only place examined where a bony overgrowth was found. Most of the excess structures can be removed and the operation continued. Deep in the cavity now exposed, the round window can be seen. Upon the membrane of this window a silver foil electrode is placed. An indifferent electrode is placed elsewhere in the skin of the animal. Both electrodes lead through a calibrated amplifier to an oscillograph or a wave analyzer (General Radio Type 736–A).

Results of the Records of Cochlear Potentials

Figure 1 represents the response curves of a typical normal rabbit for a series of eleven tones. This animal was five months of age at the time of operation and appeared to be in excellent health. The abscissa represents the intensity of stimulus in dynes per square centimeter and the ordinate expresses the response in microvolts.

[2] For review of evidence supporting view that the hair cells give rise to the cochlear response see E. G. Wever, the electrical response of the ear, *Psychol. Bull.*, 1939, **36**, 143–187.

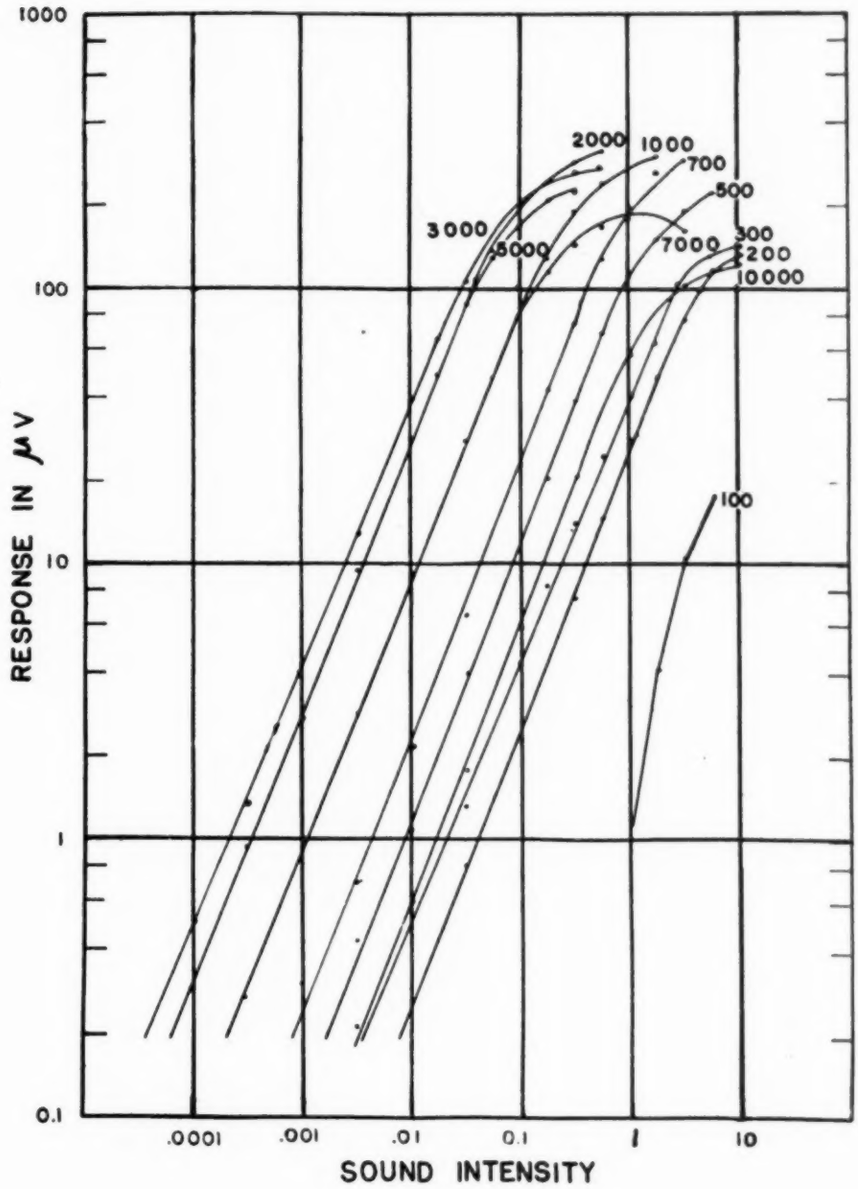

FIG. 1. Typical intensity functions for a normal rabbit. Rabbit No. 322R.

The curves show a close resemblance to those found for other species of mammals (**9**). Except at upper levels where overlading is shown, these curves are straight and have a slope of nearly unity.

In Fig. 2 similar curves are presented for a typical experimental rabbit. This animal was five months twenty-five days old at the time of operation and had been on a vitamin A-free diet for four months ten days. The lids of his eyes were very raw and the corneas were quite opaque. He had recently lost weight and had no great interest in eating. There is no significant difference between these curves for the experimental animal and those for the normal animal.

Figure 3 gives sensitivity curves for a different normal and the same experimental animal (shown in Figs. 1 and 2). The ordinate gives the strength of stimulus necessary to produce a response to

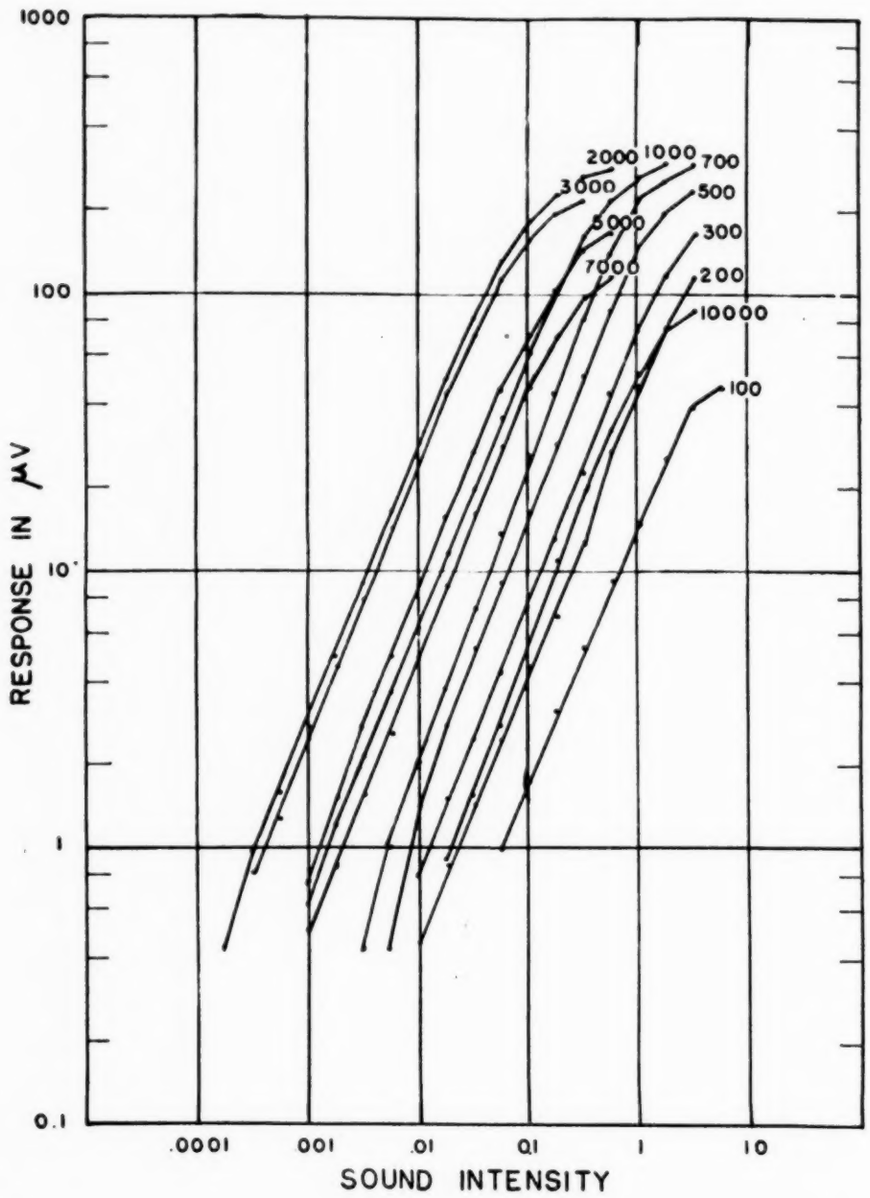

FIG. 2. Typical intensity functions for a vitamin A deficient rabbit. Rabbit No. 327R.

10 μv., while the abscissa represents frequency of stimulus. The difference between the two is too small to be considered significant. Both curves have the same shape and show maximum sensitivity in the region of 2000~ to 3000~. The curves obtained from the remaining animals, experimental or control animals alike, are similar to these.

It appears that the lack of vitamin A has done nothing to those mechanisms of the ear which give rise to the cochlear responses. Therefore, in considering the electrical responses of the rabbit to tones, both experimental and control animals can be treated as normal.

The cathode ray oscillograph was used to find the upper and lower limits of cochlear responses. For both the healthy and deficient ani-

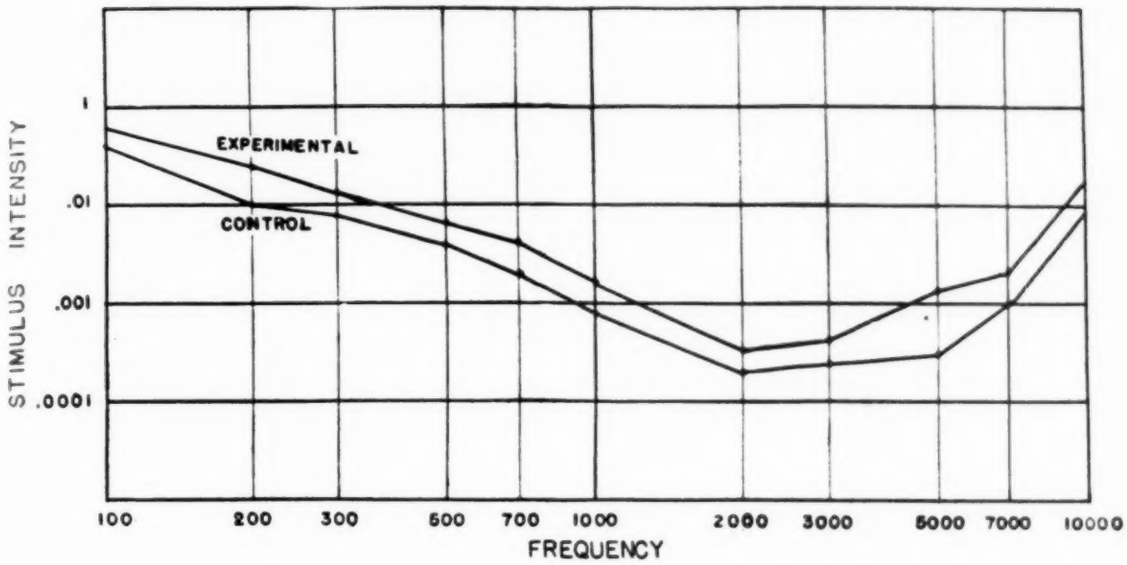

FIG. 3. Comparison of 10 μv. sensitivity curves of a normal and a vitamin A deficient rabbit. Control: Rabbit No. 326L. Experimental: Rabbit No. 327R.

mals the upper limit lay between 30,000~ and 40,000~. This is the highest limit yet found for any of the species of mammals studied (**9**).

The response to low tones showed a predominance of nerve spikes, and it is of interest here that the picture given by the ears of the experimental animals was the same as that given by the ears of the controls The cochlear response discussed above indicated the condition of structures within the cochlea. The presence of nerve spikes in the response indicates the condition of the nerve.

A series of oscillograms taken from an experimental animal are presented to show the character of the nerve responses. The responses are typical of all rabbits studied. They were taken from a rabbit so deficient that his eyes had completely dried up. His activities were confined to sitting in the corner of the cage and he was losing weight at the rate of 5 grams a day.

Figure 4 is an oscillogram recording the response of the deficient animal's ear to a tone of 1000~ presented at an intensity 30 db. below 1 dyne per square centimeter. The lower picture shows the response when the tone is increased in intensity by 10 db. The wave form distortion found at high intensity levels of stimulation, as is demonstrated in the lower picture, is typical of experimental as well as control animals.

As the stimulus is reduced in frequency the picture remains about the same until a frequency around 120~ is reached when nerve spikes begin to appear superimposed on the cochlear response. Figure 5 shows the response to stimulation by 100~. The lower picture shows the effect of an increase in stimulus intensity of 5 db.

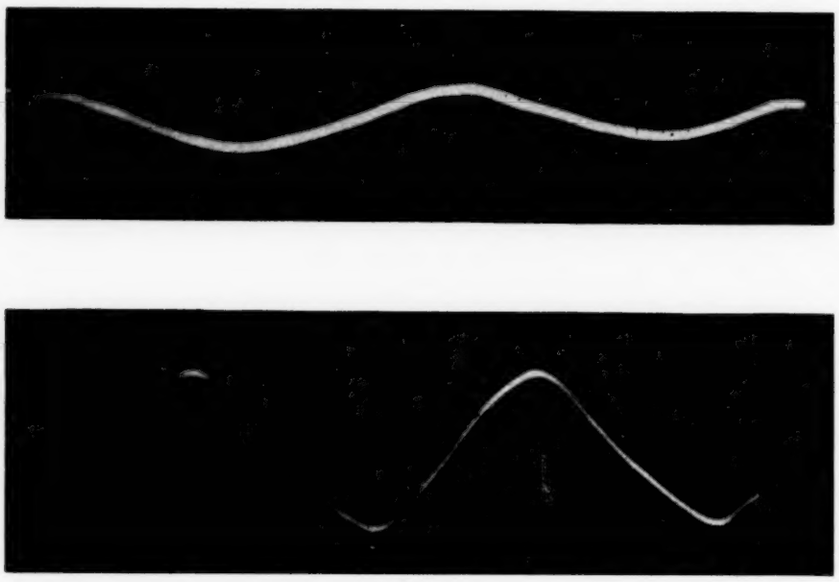

FIG. 4. (*a*) Oscillogram of response to stimulation by a 1000~ tone presented at an intensity of 30 db. below 1 dyne per square centimeter. Rabbit ear, No. 311R. (*b*) Oscillogram of response to stimulation by a 1000~ tone presented at an intensity of 20 db. below 1 dyne per square centimeter. Note wave form distortion. Rabbit ear, No. 311R.

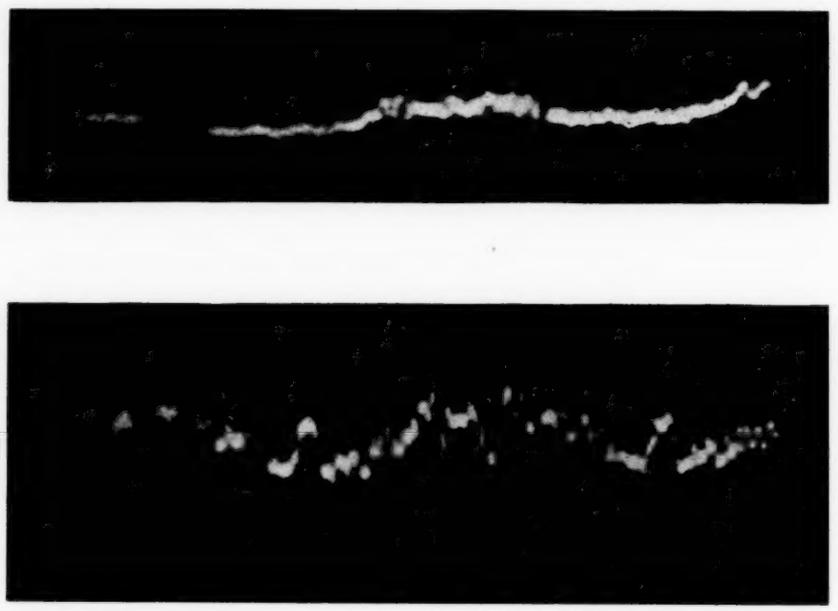

FIG. 5. (*a*) Oscillogram of response to stimulation by a 100~ tone presented at an intensity of 1 dyne per square centimeter. Rabbit ear, No. 311R. (*b*) Oscillogram of response to stimulation by a 100~ tone presented at an intensity of 5 db. above 1 dyne per square centimeter. Rabbit ear, No. 311R.

Figure 6 shows the responses to a stimulus frequency of 80~. Each lower picture represents an increase in stimulus intensity of 5 db. It can be seen that the cochlear response is beginning to disappear and that nerve spikes are becoming very prominent.

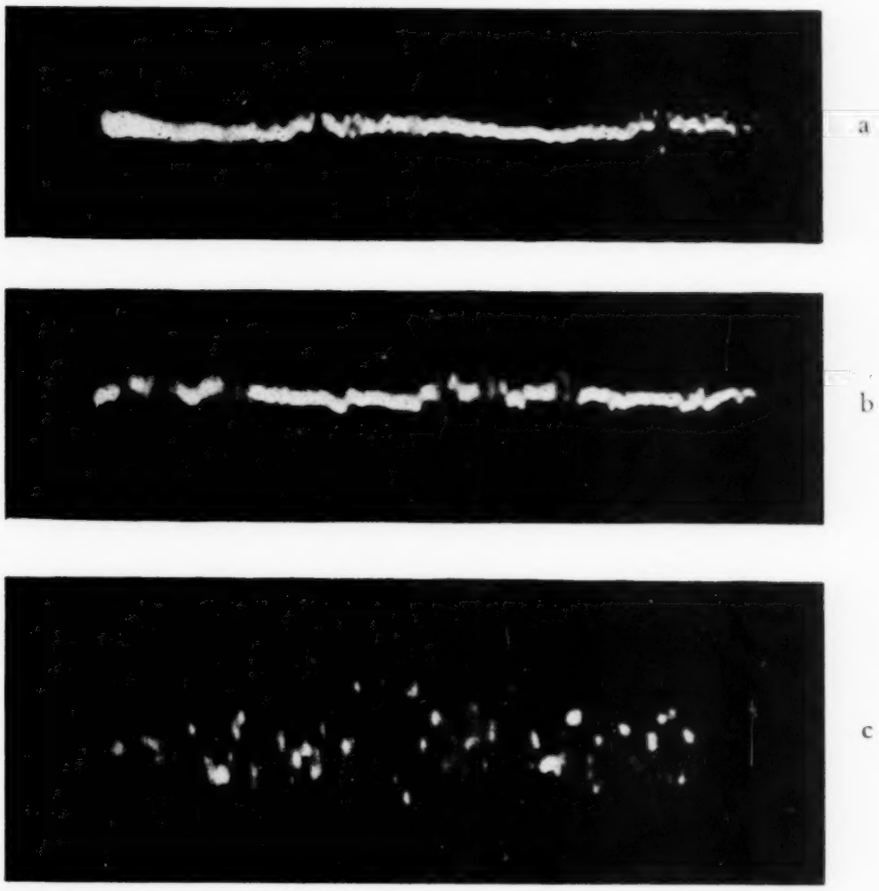

FIG. 6. (*a*) Oscillogram of response to stimulation by an 80~ tone presented at an intensity 15 db. below an arbitrary level. Rabbit ear, No. 311R. (*b*) Oscillogram of response to stimulation by an 80~ tone presented at an intensity 10 db. below the same arbitrary level used in (*a*). Rabbit ear, No. 311R. (*c*) Oscillogram of response to stimulation by an 80~ tone presented at an intensity 5 db. below the same arbitrary level used in (*a*). Rabbit ear, No. 311R.

There is no evidence in the nerve responses, as observed at the round window, of a degeneration of the VIIIth nerve in the experimental animals.

HISTOLOGICAL STUDIES

In an effort to investigate further the changes produced in the anatomy of the rabbit's ear by the lack of vitamin A, histological studies were made.

The procedure followed that developed by Guild for the guinea pig and man (**2, 3**). Fixation was obtained by perfusion through the heart of the living animal; decalcification was done slowly in 1 percent nitric acid, and the specimens were imbedded in celloidin. Serial sections were cut at 20 micra, and the sections stained in Hematoxylin and Eosin. The histological treatment required approximately five months.

As described previously, Mellanby reported numerous histological changes in his dogs fed on a vitamin A-free diet. The most prominent

of these changes was an overgrowth of bone in the modiolus of the cochlea. It was this growth that was supposed to press and stretch the nerve until it degenerated. He found changes in the cells of Scarpa's ganglion, in the sensory cells of the cristae, in the spiral

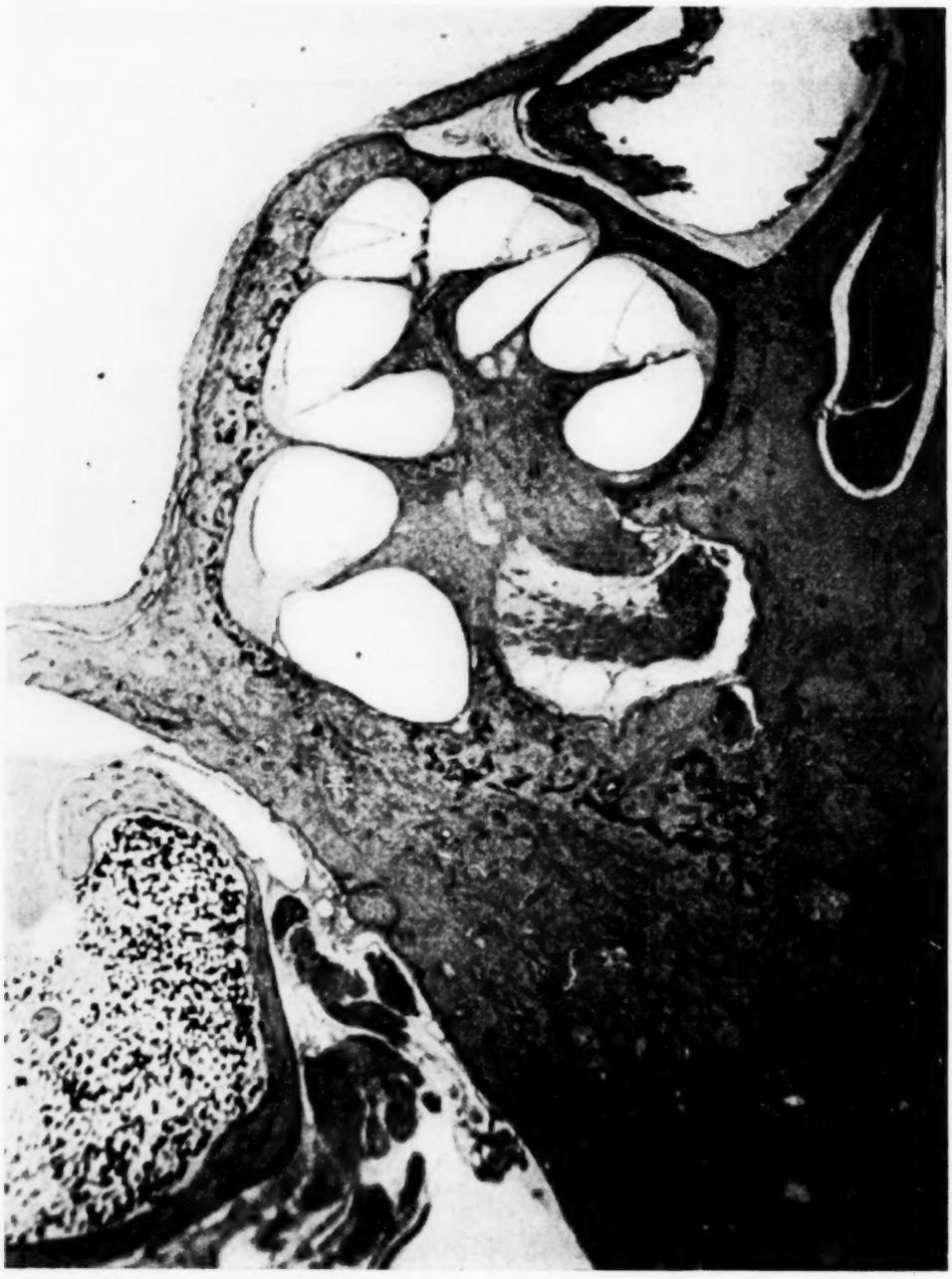

FIG. 7. Photomicrograph of entire cochlea of rabbit deficient in vitamin A. Rabbit No. 12–318–R.

ganglia, and sometimes in the hair-cells. This latter effect was supposed to be the result of serous labyrinthitis which generally occurred with severe cases of deficiency.

In the present study, no such extensive changes could be observed. Figure 7 is a picture of the cochlea of a rabbit whose diet was deficient

in vitamin A. This is the animal that had the tremors described earlier, and there can be no doubt that the deficiency was extreme. The only observations on the ear that agreed with those of Mellanby were gross overgrowth of bone at some portions of the bulla noticed during operation on the animal, and albuminoid coagula found in the scala tympani indicating a moderate serous labyrinthitis. In the picture shown, all else appears normal. There is no overgrowth of bone in the modiolus, and the cells of the spiral ganglia and Scarpa's ganglion appear normal.

Conclusions

Mellanby assumed from his study that deafness could be produced in young animals by feeding them a diet deficient in vitamin A. Though his reported observations were made on the dog, he stated that the same effect could be produced in rabbits and rats. From the results of the present investigation it seems that these conclusions must be limited.

It is well established that different species of animals require different amounts of vitamins, and hence react differently to a lack of any particular vitamin or combination of vitamins (4). This fact may account for the discrepancy between the present results and those reported by Mellanby. In short, it cannot be assumed that experimental deafness can be produced in all young animals by feeding a diet deficient in vitamin A.

Summary

A group of rabbits was fed a diet deficient in vitamin A for the purpose of studying the effect of the lack of this vitamin on hearing. To test whether any auditory impairment was produced in the animals, behavioral responses and electrical responses of the cochlea and auditory nerve were used. These measurements were followed by a histological examination to determine the extent of anatomical change.

1. Behavioral responses gave no evidence of a loss of hearing.

2. The electrical responses of the cochlea showed no difference between the normal rabbits and the ones deficient in vitamin A. The range of response for both intensity and frequency remained the same. The ears are most sensitive in the region of 3,000~. The upper limit is in the neighborhood of 40,000~, and the lower limit is below 100~, probably below 40~.

3. The records of nerve spikes appear the same for both experimental and control animals.

4. The lack of vitamin A produced no anatomical changes in the cochlea of the rabbit except serous labyrinthitis in moderate degree.

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References

1. Bensley, B. A., *Practical anatomy of the rabbit*, Philadelphia: P. Blakeston's Son & Co., Inc., 1938.
2. Crowe, S. J., Guild, S. R., and Polvogt, L. M., Observations on the pathology of high tone deafness, *Bull. Johns Hopkins Hosp.*, 1934, **54**, 315–379.
3. Guild, S. R., War deafness and its prevention, *J. Lab. & Clin. Med.*, 1919, p. 4.
4. Mathews, A. P., *Physiological chemistry*, Baltimore: The Williams & Wilkins Co., 1939, pp. 1352–1372.
5. Mellanby, E., *Nutrition and disease*, Edinburgh and London: Oliver & Boyd, 1934, pp. 112–113.
6. Mellanby, E., Lesions of the central and peripheral nervous systems produced in young rabbits by vitamin A deficiency and a high cereal intake, *Brain*, 1935, **58**, 141–174.
7. Mellanby, E., The experimental production of deafness in young animals by diet, *J. Physiol.*, 1938, **94**, 380–399.
8. Walbach, S. B., and Bessey, O. A., Relative overgrowth of the central nervous system in vitamin A deficiency in young rats, *Science*, 1940, **91**, 599–600.
9. Wever, E. G., and Bray, C. W., A comparative study of the electrical responses of the ear, *Proc. Amer. Philos. Soc.*, 1937, **78**, 407–410.
10. Wever, E. G., The electrical responses of the ear, *Psychol. Bull.*, 1939, **36**, 143–187.

THE EFFECT OF CHANGE IN TIME OF REINFORCEMENT IN THE MAINTENANCE OF CONDITIONED FLEXION RESPONSES IN DOGS

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Studies in the function of the temporal relationship of the CS[1] and US in the formation of a CR have been performed by Wolfle (12), Bernstein (1), Skinner (11), and Kappauf and Schlosbert (10). However, once a CR has been elaborated with the optimal temporal relationship between the CS and US, no investigations have been carried out to find whether variation from the optimal temporal relationship would result in the maintenance of the conditioned response. While most experimental studies in the field of conditioning have been directed toward an investigation of the conditions under which a CR may be elaborated, several recent studies have been reported which bear upon the maintenance of a CR, when the conditions under which it was established are varied. Experiments have been reported on the maintenance of conditioning with variation in the frequency of reinforcement by Brogden (3), Cole (5), and Humphreys (8, 9); with substitution of the US by a reinforcing stimulus originally neutral with regard to the UR and CR by Brogden (2); and with change in the locus of application of the US by Brogden (4). The temporal relationship of the CS and US is an important variable in conditioning, one which when altered might result in the effective maintenance of a CR already elaborated, as does change in the variables mentioned above. The object of the present investigation was to determine, with instrumental conditioned left forelimb flexion in dogs already established, the effect of change in the time of reinforcement upon the maintenance of the CR.

Experimental Procedure

Twenty mongrel dogs, taken from an unselected laboratory stock, served as the subjects in these experiments. The animals were placed in a stock within a light-shielded, sound-proofed test-chamber. The experimenter made his observations through a one-way glass screen in the wall of the experimental room. Left forelimb flexion, elicited by an electric shock to the left forepaw, was conditioned to the sound of an electric door-bell. The bell was 2 seconds in duration and was followed immediately by the shock, which lasted for 1/10 of a second. Throughout the experiment, the intensity of the shock-stimulus was such that it just evoked a noticeable flexion of the left forelimb. The stimuli were electrically controlled, with a telechron motor and suitable relays making each test-sequence identical. Twenty trials per day were given as the

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[1] Abbreviations: US = Unconditioned (original) Stimulus; UR = Unconditioned (original) Response; CS = Conditioned (substitute) Stimulus; and CR = Conditioned Response.

test-unit. Conditioned left forelimb flexion to the bell was instrumental in the avoidance of the shock. When all animals attained the criterial-level of 100 percent conditioned shock-avoiding flexions for one test-period, they were divided at random into five groups of four. The four animals in Group I were given as many shocks to the left forepaw at the *end* of each test-period as there were failures to respond to the twenty presentations of the CS during that test-period. The bell was always presented alone, shock-reinforcement being given only at the *end* of the test-period and its frequency being inversely dependent upon the frequency of CR. The four animals in Group II were given five shocks to the left forepaw at the beginning of each test-period, *before* the twenty trials of bell alone were presented. Shock-reinforcement was given only at this time, and its frequency remained independent of the frequency of CR. The four animals in Group III were given twenty trials of bell alone each test-period, no shock-reinforcement being given at any time. Hence, the animals of Group III served as controls for the animals in Groups I and II.

With the other eight animals, once the criterial level was reached, shock was omitted altogether, and after each conditioned flexion to the bell, a biscuit of food was automatically dropped in a food-box to the right of the animal at the end of the stimulus (2). If there was no conditioned flexion response to the CS, food was omitted. When a series of 100 percent conditioned flexions to bell reinforced by food was attained, these eight animals were divided into two equal groups. With the four animals in Group IV, food was automatically dropped in the food-box following each conditioned flexion to bell, but a lid on the food-box, whose release was controlled by the experimenter, prevented the animal from eating. For 100 trials (5 test-periods), the lid of the food-box was released at the end of the 5th, 10th, 15th, and 20th trials of the test-unit, and the animal was permitted to eat the biscuits of food, the number of which depended upon the frequency of conditioned flexion in the preceding five trials. For the next 100 trials (5 test-periods), the conditions were the same except that the lid of the food-box was released only at the end of the 10th and 20th trials. For 400 trials (20 test-periods), the lid of the food-box was released only at the end of the test-period, after the 20th trial. If conditioned flexion occurred to the CS, a biscuit of food was released into the food-box; if there was a failure of flexion to the bell, no food was released. Food was available only at the *end* of the test-period and its quantity was a direct function of the frequency of CR during the test-period. With the four animals of Group V, food was omitted altogether. The bell was presented alone, without there being reinforcement of any kind. These animals then, served as controls for Group IV.

Experimental Results

The conditions of shock-reinforcement used with the animals of both Groups I and II failed to prolong the duration of conditioned flexion reliably longer than that duration maintained by the animals of Group III (Table I). The animals of Group I, which received as many shocks at the end of the test-period as there were conditioned response-failures, gave a 0 percent CR score in a mean of 9.50 test-periods (σ_M = 0.87 test-period). The animals of Group III, where there was no shock reinforcement of any kind, gave a 0 percent CR score in a mean of 7.25 test-periods (σ_M = 1.25 test-periods). The difference of 2.25 test-periods between the means for Groups I and III, and in favor of Group I, when subjected to Fisher's *t* test for the significance of differences between small samples (7), is not statistically significant. Such a difference as this might be expected to occur in 10 out of 100 samples drawn at random from a homogeneous population. Nor is the difference of 4.25 test-periods between the means of Groups II and III a reliable difference. Such a difference might be expected in 8 out of 100 samples drawn at random from a homogeneous population.

TABLE I

The response-levels in percent of CR to the 20 presentations of the CS are given for each animal. For the animals of Group I, as many shocks to the left forepaw were given at the end of the test-period as their CR failures. The animals of Group II received 5 shocks at the beginning of each test-period. The animals of Group III received no shock-reinforcement whatever, and thereby served as controls for the other two groups. The means, in terms of the number of test-periods required to reach a 0 percent CR, are given at the bottom of the table, together with the standard error of the means.

Group	I				II				III			
Animal	1	2	3	4	5	6	7	8	9	10	11	12
1	100	100	100	95	100	75	100	100	50	95	50	80
2	80	100	100	100	100	65	100	100	25	35	30	85
3	85	100	70	95	95	100	100	100	5	20	30	65
4	80	90	95	70	100	80	85	100	15	0	10	65
5	90	95	75	100	85	45	55	90	5		5	40
6	90	95	10	30	65	10	65	90	10		0	25
7	70	90	0	15	5	5	40	50	10			15
8	20	80		20	10	10	0	95	10			20
9	0	30		5	5	10		45	30			0
10		5		0	0	0		30	0			
11		0						15				
12								5				
13								5				
14								10				
15								5				
16								5				
17								20				
18								0				
Mean	9.50 T-P				11.50 T-P				7.25 T-P			
σ_M	0.87 T-P				2.36 T-P				1.25 T-P			

While there are no reliable differences between the experimental and control groups in the length of time during which conditioned flexion was maintained, there were significant differences in other aspects of their performances. When one examines the curves in Fig. I, constructed by the Vincent method, representing the decrement in response for the three groups of animals, it is obvious that the curves for Groups I and II show a greater frequency of response for the early points than does the curve for Group III. Since each point on each curve represents the mean performance of four animals, the differences between the points of the curves for Groups I and III, and for Groups II and III, were subjected to Fisher's *t* test. The first eight points on the curve for Group I are significantly greater than the respective points for Group III. The first five points of the curve for Group II are significantly greater than the corresponding points for Group III. In all cases, the *p* values of the differences were .05 or less. The conditions of shock-reinforcement for the ani-

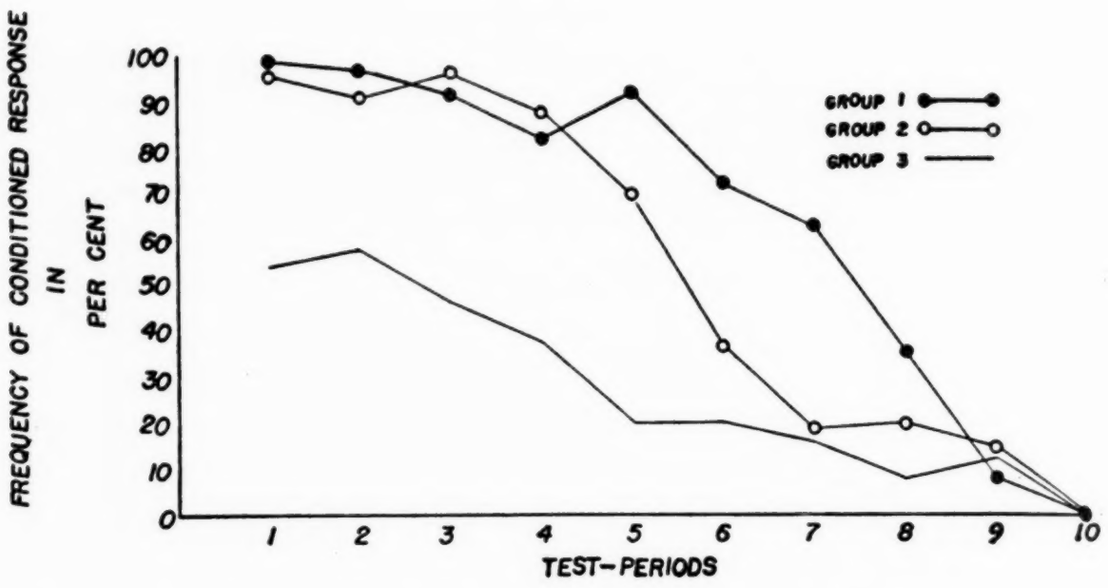

FIG. I. The first point on each of the curves represents the mean frequency of conditioned response during the first tenth of the trials before o percent response; the second point represents the mean frequency of CR during the second tenth; and successive points represent mean performances of successive tenths of the trials. The animals of Group I, their performance represented by the filled circles, were given as many shocks at the end of each test-period as there were CR failures. The animals of Group II, their performance represented by the open circles, were given five shocks at the beginning of each test-period. The animals of Group III, their performance represented by the small dots, were given no shock reinforcement of any kind.

mals of Groups I and II were effective in maintaining a higher frequency of conditioned response during the early periods than that of the animals of Group III under no shock-reinforcement.

There were also marked differences in the general behavior of the animals in the experimental and control groups. The control animals, Group III, with no form of shock-reinforcement, were quiet in the test-room, made no flexion-responses at random, and whenever a conditioned flexion-response to the bell occurred, it was clear-cut and discrete. The animals of Group I were restless when they were placed in the test-chamber; flexion-responses of all limbs occurred at random; vocal responses were frequent. The intensity and frequency of these responses increased as the time approached at which the shocks were given. This general picture of these animals is not so true of the early test periods as it is of the later ones. The animals of Group II, where five shocks to the left forepaw were applied at the beginning of each test-period, were restless, barked, made random flexions of all limbs, and frequently held the left forelimb flexed, when placed in the test-situation. After the five shocks had been delivered, this behavior disappeared; the animals were quiet, and whenever a CR was evoked by the bell, it was sharp and discrete. This description of the general behavior of the animals of Group III applies more to the later test-periods than it does to the early ones.

When the lid of the food-box was released after the 5th, 10th, 15th, and 20th trials, and the animals of Group IV were permitted to eat as many biscuits as there were CR's, the frequency of CR was high (Table II). The mean frequency of CR for the 100 trials under

TABLE II

The response-levels in percent of CR to the 20 presentations of the CS are given for each animal. The first section of the table gives the response-scores for the animals of Group IV when food was available at the end of the 5th, 10th, 15th, and 20th trials; the second section when food was available after the 10th and 20th trials; and the third section when food was available at the end of the 20th trial. In each case, the amount of food was a direct function of the frequency of response in the preceding trials. The animals of Group V received no food-reinforcement of any kind, and thus served as controls for Group IV. Means for each group are given for each experimental condition. The standard error of each mean is given in parentheses.

Group	IV				V			
Animal	13	14	15	16	17	18	19	20
1	100	100	90	100				
2	100	100	100	80				
3	100	90	100	80				
4	90	65	100	70				
5	100	100	90	100				
Mean	92.75% (2.45%)							
1	100	95	90	65				
2	90	95	70	85				
3	100	65	50	50				
4	95	80	65	90				
5	95	85	90	65				
Mean	81.00% (3.60%)							
1	100	100	85	35	100	55	10	85
2	100	70	65	35	90	40	40	80
3	100	75	75	55	80	25	65	65
4	100	85	75	50	60	10	30	30
5	100	85	85	35	45	15	10	70
6	95	85	75	50	20	0	30	75
7	95	90	75	85	0		0	50
8	90	70	80	60				45
9	85	75	70	50				20
10	20	90	50	40				25
11	0	65	60	15				10
12	5	60	55	55				0
13	5	45	75	45				
14	0	35	90	30				
15	0	5	75	15				
16	0	50	70	15				
17	0	70	35	65				
18	10	30	60	45				
19	0	15	50	20				
20	0	0	60	15				
Mean	17.75 T–P (2.25 T–P)				8.00 T–P (1.35 T–P)			

these conditions for the group was 92.75 percent (σ_M = 2.45 percent). When food-reinforcement occurred at the end of the 10th and 20th trials, the mean response-frequency for the group was 81.00 percent (σ_M = 3.60). For the 400 trials where food was available only at the end of the 20th trial, the mean response-frequency for the group was 53.56 percent (σ_M = 2.99 percent). With the decrease in the frequency with which the lid of the food-box was raised, and with time, there was a progressive decrease in the frequency of response. As for the duration of conditioned flexion under these conditions of reinforcement, animal 13 gave a 0 percent response on the 11th test-period, animal 14 gave a 0 percent response on the 20th test-period, while the remaining two animals of Group IV were still responding on the 20th test-period (Table II). Since the experiment was not continued until animals 15 and 16 failed to respond, no accurate group score can be computed to represent the duration of response under delayed food reinforcement. However, since the duration of CR for these two animals did last for 20 test-periods, an underestimated mean can be computed using a score of 20 for them. The mean of 17.75 test-periods, representing the duration of CR for the animals of Group IV, when as many biscuits of food were presented at the end of the test-period as there were CR's to the CS, can be compared with the mean of 8.00 test-periods, which represents the duration of CR for the animals of Group V without food-reinforcement of any kind. The difference of 9.75 test-periods (σ_M = 2.58 test-periods), in favor of Group IV, when subjected to Fisher's *t* test, is statistically significant. Such a difference might be expected by chance only 4 times in 1000 samples drawn at random from a homogeneous population. The conditions of reinforcement used with the animals of Group IV maintained a higher frequency and a significantly longer duration of CR than did the conditions of no reinforcement with the animals of Group V.

Discussion of Results

The two modes of shock-reinforcement that were used with the animals of Groups I and II did not result in a significantly longer maintenance of conditioning than did the lack of reinforcement with the control animals, but during the early part of the experiment they were effective in maintaining a greater frequency of conditioned response with the experimental animals than with the controls. Why was this initial reinforcing effect of grouping the shocks either at the beginning or end of the test-period ineffective during the latter part of the experiment? The grouping of the shocks at the beginning or end of the test-period at first served to reinforce conditioned

flexion to the ringing of the bell. But shocks grouped in this manner also served as unconditioned stimuli for agitated behavior on the part of the animals. This restlessness, randomness of flexion, and vocalization became conditioned to the general aspects of the experimental situation, particularly to the temporal duration of the experimental period. With the development of this conditioned behavior, forelimb flexion to bell, not reinforced, suffered extinction. Conditioned agitated behavior, reinforced, became established. It is possible that if the shock-reinforcement used had been less frequent, and hence less intense, there would have been no agitated behavior. Without the agitated behavior being evoked, it could not be conditioned. In these circumstances, it may be that shock-reinforcement might have maintained conditioned flexion to the bell significantly longer than no shock-reinforcement.

Unfortunately, the conditions of reinforcement used with the animals of Group IV are complex. The presentation of as many biscuits of food at the end of the test-period as there were CR's to the bell is not the only variable in the experimental situation. Whenever a CR was made to the CS, a biscuit of food was automatically released and fell into the food-box. Because of the lid on the food-box, the animal was unable to obtain it immediately, and was forced to wait until the end of the test-period when the lid of the food-box was raised. The sound of the food-release mechanism and of the food dropping into the box after each CR undoubtedly served as a reinforcing stimulus, since this sound was paired with the eating of food in the earlier training trials. Finch and Culler (**6**) have shown that dogs conditioned to forelimb flexion required a mean time of 14.9 test-periods when a buzzer (which during training sounded for the exact duration of the electric shock) was present and 4.00 test-periods when it was absent. The sound of the buzzer had for a time taken over the incentive-function of the US, that of providing the incentive or drive for response. An additional control group should have been run so that the reinforcing effect of the delayed food reward could be isolated from that of the sounds of the food dropping and the release mechanism which occurred immediately after a CR to the CS. Since this was not done, it is impossible to assess the relative values of these two variables in the performance of the members of Group IV.

From the results obtained by Finch and Culler (**6**), it seems likely that the sounds of the food falling and of the food release mechanism played a significant role in the maintenance of conditioning with the animals of Group IV when food-reinforcement was delayed. Probably delayed food-reinforcement by itself would be no more effective in maintaining conditioned flexion to the bell than were the two conditions of shock-reinforcement. Thus the temporal relationships

between the conditioned and reinforcing stimuli used in this experiment are relatively ineffective in maintaining conditioning. Temporal contiguity of the conditioned and unconditioned stimuli is just as important in maintaining a conditioned response as it is in establishing one.

Summary of Conclusions

1. Conditioned forelimb flexion responses were maintained no longer when five shocks were given at the beginning of the test-period or when as many shocks were given at the end of the experimental period as there were failures of response, than when there was no shock-reinforcement.

2. However, under these conditions of shock-reinforcement, there was a significantly greater frequency of CR during the first half of the experiment than with no shock-reinforcement.

3. The initial reinforcing effect of shock under these conditions did not persist because the presentation of shock at either the beginning or the end of the test-period evoked agitated behavior. This agitated behavior became conditioned to the general experimental conditions. Conditioned agitated behavior was reinforced by the methods of shock-presentation, whereas conditioned flexion to the bell was not. Conditioned flexion was thereby extinguished while conditioned agitated behavior became established.

4. It is suggested that if the conditions of shock-reinforcement had been such that agitated behavior was not elicited, conditioned flexion might have been maintained significantly longer than conditioned flexion without any form of shock-reinforcement.

5. Conditioned flexion was maintained at a higher frequency and significantly longer when food-reinforcement was delayed until the end of the test-period than when there was no food-reinforcement.

6. Because of evidence obtained by Finch and Culler (**6**) it is likely that the sound of the food release mechanism and of food falling immediately after a CR, played a significant reinforcing role. It was impossible to assess the relative values of this factor and the delayed presentation of food in the maintenance of conditioned flexion.

7. The temporal relationships between the conditioned and reinforcing stimuli used in these experiments are relatively ineffective in maintaining conditioning.

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References

1. Bernstein, A. L., Temporal factors in the formation of conditioned eyelid reactions in human subjects, *J. gen. Psychol.*, 1934, **10**, 173–197.

2. Brogden, W. J., Unconditioned stimulus-substitution in the conditioning process, *Amer. J. Psychol.*, 1939, **52**, 46–55.

3. Brogden, W. J., The effect of frequency of reinforcement upon the level of conditioning, *J. exp. Psychol.*, 1939, **24**, 419–431.
4. Brogden, W. J., Conditioned flexion responses in dogs reestablished and maintained with change of locus in the application of the unconditioned stimulus, *J. exp. Psychol.*, 1940, **27**, 583–600.
5. Cole, L. E., A comparison of the factors of practice and knowledge of experimental procedure in conditioning the eyelid response of human subjects, *J. gen. Psychol.*, 1939, **20**, 349–373.
6. Finch, G., and Culler, E., Relation of forgetting to experimental extinction, *Amer. J. Psychol.*, 1935, **47**, 656–662.
7. Fisher, R. A., Application of 'Student's' distribution, *Metron*, 1925, **5**, 90–104.
8. Humphreys, L. G., The effect of random alternation of reinforcement on the acquisition and extinction of conditioned eyelid reactions, *J. exp. Psychol.*, 1939, **25**, 141–158.
9. Humphreys, L. G., Generalization as a function of method of reinforcement, *J. exp. Psychol.*, 1939, **25**, 361–372.
10. Kappauf, W. E., and Schlosberg, H., Conditioned responses in the white rat: III. Conditioning as a function of the length of the period of delay, *J. gen. Psychol.*, 1937, **50**, 27–45.
11. Skinner, B. F., The effect on the amount of conditioning of an interval of time before reinforcement, *J. gen. Psychol.*, 1936, **14**, 279–295.
12. Wolfle, H. M., Conditioning as a function of the interval between the conditioned and original stimulus, *J. gen. Psychol.*, 1932, **7**, 80–103.

INDUCTION AND RECOVERY SEQUENCE AFTER ANESTHETIZATION IV. PURE GASOLINE

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I. Introduction

In the earlier series of experiments ordinary anesthetics were used that were grouped under that category. The results of these studies exhibited behavioral tendencies and sequential patternings, of a caudo-cephalad induction, and a cephalo-caudad recovery (**1**, **2**, **3**). These researches all posited similarity to developmental studies on foetuses of all geni, infra-mammalian, mammalian, infra-human, and human. None of these experiments, however, used the methods described in this paper; or any of the usual anesthetizing agents. All the anesthetics were administered in the volatile state. Since specific behavioral mechanisms were noted, certain questions arise: were they typical of any one group or of all groups of hydrocarbons and their derivatives? Or could similar pertinent behavioral patterns be elicited from some product never before considered in the light of an anesthetic—gasoline? It had to be the pure substance, and not adulterated with any poisonous ingredients; nor should any of the combustible products be introduced. If these behavioral patternings did occur now, would they give any final or salient clue to the theoretical explanation of all early sequential patternings? Was there any ready answer to developmental progress? Or would it still remain entirely one of individualistic interpretations? These were a few of the questions which arose that seemed essential. Emphasis on exploratory stimulation by means of any mechanically controlled manipulaton was relegated to a lesser position of importance. Pure volatized gasoline was used and not the combustion products. The results that followed appeared singularly significant.

II. Physical Properties of Gasoline, and some of its Physiological Effects

The petroleum, or aliphatic series, contains a large number of compounds, whose distillates are mixtures of hydrocarbons of the formula CH_4 to $C_{35}H_{72}$. They have boiling points varying from 30° C to more than 300° C. It is under this classification that benzine, naphtha, and gasoline belong. Gasoline is primarily made up of hexanes and pentanes.

Experiments as to the toxicity of petrol fumes on animals have been few; but those that have been done show a direct relationship to the results found among human beings. Poincare (4) demonstrated that lethal and narcotic dosages of gasoline for the rabbit, administered by an intravenous injection and also orally, varied from 20 cc per Kg body weight to 15 to 20 grams. A tolerance for this hydrocarbon derivative was especially evident. Matsushita (5) produced symptoms similar to an acute narcotic poison; namely, twitching, colonic convulsions with restlessness, dyspnœa, and in several cases exophthalmos. He placed special stress on the histological examination of the brain and the spinal cord; and concluded that gasoline tended to destroy nerve cells in the brain cortex, medulla oblongata and the spinal cord. Poincare (4) showed that the inhalation of the vapor had the following effects: agitation, somnolence, loss of appetite, but that these effects were not fatal. Furthermore, some animals, such as dogs, showed some changes in their circulatory and respiratory rhythms; the rabbit exhibited the same effects, and, in addition, some incoördination of movement and somnolence; with guinea pigs, there were loss of appetite and weakness that resulted in death.

In other experiments, namely, on the human being, mere observations were noted in factories where industrial solvents were used. These experiments were not controlled, nor were the details specific. Apparently a toxicity that resulted from inhalation was due to the admixture of the several hydrocarbons, and to the fact that the workers were persistently and continually in contact with the vapors. Other observations were noted among workers in petroleum mines and in ships carrying the product. The syndrome that seemed prevalent among such subjects was lassitude, somnolence, a heaviness in one's head, noises in the ears, and later a heavy, deep sleep—the other characteristic symptoms of any acute form of gasoline poisoning were similar to those of alcoholic imbibition, namely, a restlessness, excitement, and hilariousness. The other extreme was also enacted —irritability, a desire to fight, and tears. There have been cases in which girls working at industrial plants evinced a "jag," with a hyperexcitability, excess laughter, and sometimes a hysteria. By means of this same observational method of experimentation, painters are said to have experienced a spottiness before their eyes, a cloudiness of vision, and a strange sensation around their waist. At no time, however, was the pure gasoline product used, but rather complex derivatives of the whole hydrocarbon series. The method of judgment of details upon the human being were more anecdotal than scientific.

III. Materials, Subjects, Procedure

Heterogeneous groups of white rats, both male and female, were used throughout this research. Their age range varied from 2 to 12 months. Random selection of any control group was considered unnecessary, since the results were in uniformity. None of these animals had ever been used in any of the other anesthetization experiments, so that any tolerance factor for anesthetics was eliminated. The age differentiation did not appear important. Since the same method of selection had been used previously, it was again adopted. All areal and punctiform stimulation was eliminated as being irrelevant and artificial.

The animal was placed in a glass anesthetization cabinet, in which were placed several pellets of absorbent cotton saturated with approximately 5 cc of a pure gasoline. The cotton had been compressed to near dryness, so that the animal would absorb only the fumes from the anesthetizing apparatus. Air was allowed to enter the chamber so that normal respiration could take place, and to enable the gasoline to evaporate at the normal body metabolism of the animal, as well as to eliminate the possibility of asphyxiation that would result from any disruption of a CO_2 and O_2 balance. The animals were left in the anesthetization cabinet for variable time limits of 4 to 7 minutes. This seemed necessary, in order to assume a criterion of anesthetization, or near anesthetization; namely, somnolence and inactivity. The group numbered 20 animals; and any larger group selection after this study was completed seemed trivial.

Tables 1 and 2 present a summary list of the sequential and behavioral patternings of the animals. These group segmentations were used only to show the interrelationship of these patterns, and as a convenience for behavioral descriptions, none of which was meant to construe inflexible demarcations. They were convenient categorical representations of the animals' behavioral tendencies.

A protocol of the typical animal can best demonstrate the progressive locomotor sequences upon anesthetization through the inductive to the recovery state.

Animal 8 was 10 months of age, and was placed into the glass anesthetization cabinet. As gasoline began to volatilize, the animal became hyperkinetic and its movements were extremely generalized. There were rotation of his body, a sniffing, some sneezing, and then a slight pause. He began to move about, but less actively. He tried to sit upon his haunches and attempt escape, but fell forward. All of this action varied for one minute and nine seconds. He again attempted a rotatory movement, but stopped shortly. His hind limbs appeared deadened, and disequilibration was present. Several abortive attempts to raise himself resulted in his falling forward. He pawed with his front legs, and his head moved from side to side. His heart beat became sharp and accentuated; and his abdominal muscles seemed to contract and then relax. The movement of his front legs ceased, and now only his head evinced some perceptible motion. The vibrissae action was lessened; and his breathing became labored. All diffuse activity and specific motion was reduced. There was a slight tremor of his front right leg, a slight convulsiveness. The animal was now apparently inactive after a period of 4 minutes and 35 seconds. Inductive anesthetization had taken place.

TABLE 1

INDUCTION SEQUENCE

Rat Number	Defecation	Micturition	Diffuse body activity of whole structure	Rotation of body	Loss of use of hind limbs	Disequilibration	Loss of use of fore limbs	Disappearance of head movement	Vibrissae movement	Slight proximal leg tremor
I	1	2	3	4	5	6	7	8	9	
II	2	1	3	4	5	6	7	8	9	10
III		1	2	3	4	4	5	6	7	
IV	1	2	3	4	5	6	7	8		9
V	1		2	2	3	4	5	6	7	8
VI	1		2	3	4	5	6	7	8	9
VII	1	2	3	4	5	6	7	8	9	10
VIII	1	2	3	4	5	6	7	8	10	9
IX		1	2	2	3	3	4	5	6	7
X	1	2	3	4	5	5	6	7	8	9
XI	1		2	3	4	5	6	7	8	
XII	1	2	3	4	5	6	7	8	9	10
XIII	1		2	3	4	5	6		7	8
XIV	1	2	3	3	4	5	5	6		7
XV		1	2	3	4	5	6	7	8	9
XVI	1	2	3	4	5	6	7	8	9	10
XVII	1	2	3	4	5	6	7	8	9	10
XVIII		1	2	2	3	4	4	5	6	7
XIX			1	2	3	4	5	6	7	8
XX	1		2	3	4	5	6	7	8	

TABLE 2

RECOVERY SEQUENCE

Rat Number	General flaccidity	Slight convulsive tremor of front and hind leg	Vibrissae movements	Head alone motion	Movements of head and fore limb	Movement of hind limb	Activity of head, fore limb, and hind leg	Righting posture	Locomotion with Incoordination	Ipsolateral movement	Equilibration	Defecation
I	1	2	3	4	5	6	6	7	8		9	
II	1	2	3	4	5	6	7	8	9	10	11	12
III	1	2	3	4	5	7	6	8	9	10	11	
IV	1	2	3	4	5	6	6	7	8	9	10	11
V	1	2	3	4	5	6	7		8	9	10	11
VI	1	1	2	3	4	5	6	7	8		9	10
VII	1	1	2	3	4	6	5	7	8	9	10	
VIII	1	1	2	3	4	5	5	6	7	8	9	
IX	1	1	2	3	4	5	6	7	8		9	10
X	1	1	2	3	4	5	6	7	8		9	10
XI	1	2	3	4	5	6	7	8	9		10	
XII	1	2	3	4	5	6	6		7	8	9	10
XIII	1	1	2	3	4	5	6	7	8		9	
XIV	1	2	3	4	5	6	7	8	9	10	11	12
XV	1	1	2	3	4	5	5	6	7	8	9	10
XVI	1	1	2	3	4	5	6		7	8	9	10
XVII	1	1	2	3	4	5	6		7	8	9	10
XVIII	1	1	2	3	4	5	6	7	8	9	10	11
XIX	1	2	3	4	5	6	7	8	9	10	11	
XX	1	2	3	4	5	6	7	8	9	10	10	

The animal was then removed to the operating table, where his further motile behavior mechanism was observed. His breathing was short and hard. Salivation was apparent. The animal remained inert, and the only observable behavior was the tremors of both front and hind limbs. These, however, were completely anesthetized, for, upon being prodded, they remained in their original position; there was no withdrawal. The animal's breathing began to resume normalcy, head movements were elicited, then head and front legs, followed by a perceptible extension of the hind leg. The animal now attempted to right itself, but fell over on its side. After repeated attempts, the animal was in a prone position. Some forward locomotion resulted in a rocking and weaving motion. The animal appeared to be dragging its hind limbs. There followed a rest period of 10 seconds, and again forward motion was attempted. The animal moved forward, weaving, toppling over, righting itself; but evinced complete non-equilibration. Coördination of the hind and fore limbs was lacking. The proximal position of the animal's body seemed to maintain some steadiness, while the caudal portion of the body was disequilibrated, the result being a figure S type of walking. After a period of rest, the animal raced along the table, using one side more than the other. Although the animal appeared to have recovered the use of his propensities for movement, there was a partial ipso-lateral recovery. Animal 8 recovered completely in 7 minutes. After this locomotor progression, the animal was as active as ever; and unlike the other early anesthetic groups of other research studies, ate ravenously and drank thirstily. There were no visible signs of any toxicity, or any subsequent indications. Later observations confirmed these results.

IV. Sequences During the Induction and the Recovery Following Gasification

Each of the animals of this experimental study similarly displayed a general type of body activity and locomotion; the loss of motor function upon induction was in the caudo-cephalad direction, while their recovery was essentially cephalo-caudad. Sometimes some diffuse motor idiosyncrasy occurred simultaneously with another; these were noted by the same numerals in the charts. But the composite of the behavioral and sequential patternings is best indicated in a synoptic summary.

A. Induction Sequence

1. Defecation
2. Micturition
3. Diffuse body activity of the whole structure

4. Rotation of body
5. Loss of use of the hind limbs
6. Disequilibration
7. Loss of use of fore limbs
8. Disappearance of head movements
9. Vibrissae movements
10. Slight proximal leg tremor

B. Recovery Sequence

1. General flaccidity
2. Slight convulsive tremor—front leg and hind leg
3. Vibrissae movements
4. Head alone
5. Head and fore limb
6. Hind limb
7. Head, fore limb, and hind limb
8. Righting posture
9. Locomotion with incoördination
10. Ipso-lateral movement
11. Equilibration
12. Defecation

V. Summary and Conclusions

Since all the other anesthetics in previous experiments had been administered in a volatile gaseous state, it was necessary to use a similar method of gasification. By means of this method, would gasoline with this controlled procedure follow the same sequences of anesthetization as did the other officially recognized anesthetics or was it solely a feature of the latter? Gasoline did have marked anesthetic properties; consequently, similar patternings of behavioral tendencies in their appearance and their disappearance were exhibited. There was a clear order in the unfolding of these locomotor patternings. The temporal spacing of this whole mechanism was caudo-cephalad and cephalo-caudad. The animal was never dichotomized into segmental units, but allowed to remain a functionable and dynamic totality. The progressiveness of the sequential patternings was visibly caudo-cephalad upon induction, and cephalo-caudad upon recovery. There were no variations, and the results were consistent. Only one peculiar occurrence should be noted; namely, that there was an ipso-lateral recovery, after the head, front and hind limbs had recovered. This fact, however, did not detract from the efficacy of the results. The recovery sequence did not vary from its cephalo-caudad progressiveness. The tremors that were also a part

of the animal's behavior were apparently induced by the pharmacological and blood chemical action of gasoline, plus the dosage used. However, the patterning picture and the sequence mechanism was not offset by these occurrences.

No mechanical measures for bodily exploration were used. The body was never mapped out into infinite segmental areas for punctiform or areal stimulation to uncover elemental "reflexes." The sequential patternings had occurred without any artificial instrumentation.

Animals of these various age groups showed the same behavioral patterns. Age itself did not seem to be an important factor for the occurrence of the unfolding pattern—the young animal and the older one displayed no variability. Sex, too, did not seem to enter into the efficacy of the results. Consequently, these factors, as being relevant for any sequential patterning, can be eliminated. Since these types of behavior bore a very clear relationship to much work that had been investigated in foetal and post-natal developmental sequences, it was assumed that this study might offer some theoretical explanation for all these tendencies. At the outset, it must be stressed that this study does not pretend to be a panacea for all developmental sequential work already completed, nor does it maintain that any real explanation can answer every situation that has arisen. It is reasonably certain, though, that in this work there is a series of sequences in the behavior of the animal that is cephalocaudad and caudo-cephalad. Besides, in the early studies by investigators using other techniques, a similar sequence of locomotion was disclosed. Holding this as the original premise, and following some logical interpretation, it first must be clearly understood that the animal is a functionable symbolic whole, an entity acting as a coördinate mechanism, not merely an automaton of discrete reflexes. In short, animals are more than mere reflexological constructs and therefore they cannot be divided into separate partitive wholes. The body of the animal cannot be subdivided into zones, and these in turn stimulated to evince sequences of behavioral patternings. The investigators who hope to, and do, induce these general patterns of behavior by these methods forget the anatomical and neurological structural relationship of the animals (**6**, **7**). By their procedure they may be involving the same neural mechanisms, which, however, are supplying different muscle groups. Furthermore, there is an overlapping of muscle and neural groups, and therefore one small area cannot be denoted as intrinsic for any one specific type of behavior. Likewise, stimulation of areas proposes immediately that the investigators are preparing to look for some specific type of behavior pattern. Is it too much to assume, therefore, that they have

become set and prejudicial in their investigations with the result that what they wish to find and to disclose is readily apparent? If so, they find what they are looking for, but nothing beyond that. By such methods the animal no longer is a totality but only a sum of reflexological entities. Any method of stimulation, no matter how precise, induces enough errors in its manipulation. The instrumentation is unable to move rapidly enough to produce similar temporal sequences. All areas must be stimulated precisely at the same moment; and those areas can be thus stimulated only if there are as many investigators working on the *same* organism at the *same* time, and applying the *same* pressures, as there are subdivided units. If this procedure were feasible, then the results would be of value. But it is not, and consequently, every method of stimulation on the present research was eliminated.

Myelination, too, was considered as a possible factor for all of these sequential patternings. But in all the studies that were investigated by anesthetics, in the very young, where myelination was not yet complete, as well as in the old, where it was; the *same* patterning was apparent. Myelination, then, could not appear as the sole and direct factor for any of these behavioral tendencies. Even the generalization that these tendencies may be due to inherited characteristics seems to be unsubstantiated. The theory of genes, and the inheritance of acquired characteristics may have a useful niche in some scientific endeavor but they would hardly explain developmental sequences in their entirety. It appears, therefore, that each investigator appeared to be groping for one, or some single, causative factor for all of these developmental and sequential patterns.

Since this study shows the same generalized pattern as all other developmental studies, it does not give any one specific and inflexible explanation for its occurrence, or for any work that has preceded this. The factors that seem to be involved are not one, but *many*. Structurally the animal is important; anatomically, embryologically, neurologically, physiologically, and bio-chemically. The chemistry of blood, as well as its physiological function, and its neurons correlates are important. Besides these qualities, psychologically, the factor of pre- and post-natal maturation is important in all this behavior. It must be remembered, also, that the extrinsic environmental conditions of the living organism is of some consequence. Therefore, there must be synthesis of all these factors. Each one is dependent upon the other, not only in this research study, but in all of them. The animal has to be regarded as a functionable unit, composite and dynamic, but never as a chain of reflexes that can be prodded to activity by the mere pressure of one isolated area. Dogmatism in

this science should be studiously avoided; if not it becomes a pseudo science.

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References

1. Cornsweet, A. C., Recovery sequence after anesthetization. I. Ethyl ether, *J. comp. Psychol.*, 1938, **26**, 209–216.
2. ——, Recovery sequence after anesthetization. II. Cyclopropane and nitrous oxide, *J. comp. Psychol.*, 1938, **26**, 387–395.
3. ——, Recovery sequence after anesthetization. III. Ethyl alcohol, *J. comp. Psychol.*, 1939, **27**, 165–173.
4. Poincare, L., Recherches experimentales sur les effets d'un air chargé de vapours de petrole, *Ann. Hyg. Publication, Paris*, 1885, **13**, 342.
5. Matsushita, R., Pathologisch-histologische Studien über das Zentral-nervensystem bei-experimenteller Petroleumvergiftung, *Abst. in Zbl. Gew. Hyg.*, 1935, **22**, 190.
6. Greene, E. C., Anatomy of the rat, *The American Philosophical Society*, 1935, *Phila.*, 370 pp.
7. Donaldson, H. H., The rat: data and reference, *Memoir of the Wistar Institute of Anatomy and Biology*, 1924, *No. 6, Philadelphia.*

MOTIVATION IN LEARNING: IX. THE EFFECT OF INTERPOSED OBSTACLES IN HUMAN LEARNING

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The present experiment is an attempt to determine the effect upon learning efficiency when an obstacle is interposed between the goal and human subjects as learners.

Theoretically, anything—mere distance, for instance—which separates the learner from the goal must be considered as an obstacle. Careful terminology would require a reference to something like the 'degree of difficulty' of the path to the goal rather than to the presence or absence of an obstacle. However, in order to save the reader the annoyance of a new vocabulary we have decided to adhere to an older and looser vocabulary, especially since it does not seem to lead us into confusion in the present report. We shall speak of learning situations with and without obstacles.

In considering the effects of obstacles between a learner and his goal we must deal with two factors, the strength of motivation and learning efficiency, which are probably causally related. As regards the first one, we may refer to the common observation that an obstacle may increase the desirability of a goal and consequently make the animal strive harder to reach it. An experimental demonstration of this fact has been given by Wright (**10**), who found that in the case of children and college students increased distance of a toy or some food enhanced the desirability of such goal objects.

The effect of different obstacles on variations in learning efficiency has been definitely demonstrated with white rats as subjects. The outstanding example is the superiority of Lashley's jumping technique as compared with the usual type of discrimination apparatus (**4**). Using an apparatus somewhat similar to Lashley's, Honzik and Tolman recently found (**3**) that the gap to be jumped must not be too far removed from the goal if it is to function as a facilitating factor. This, in a way, confirms our results that a gap before the point of choice has no facilitating effect (**8**).

In a direct attack upon the problem of obstacles, using white rats as subjects, we found that a gap in the floor after the point of choice in a T-shaped discrimination box has the same accelerating effect upon learning as has electric shock, and that both gap and shock are superior to mere distance between the choice point and the goal (**8**). Observing how the animals paused at the point of choice whenever

an obstacle loomed ahead, we restrained a group of rats for a few seconds at the choice point by means of glass partitions and found that such a mechanically enforced delay also has an accelerating effect (**9**).

In the present experiment it was our intention to do with human subjects what we had previously done with white rats, namely, study the effect of obstacles upon learning efficiency. Thus we duplicated in a human learning situation the four conditions, no obstacle (mere distance), obstacle, shock, and delay, previously used with white rats, in order to see whether they were similarly effective with both types of subjects. Primarily we were concerned with the effects of an obstacle added to mere distance upon learning efficiency. We used the other two conditions, shock and delay, because we had found that in white rats they were similar in function to the obstacle.

Perhaps a word of explanation is needed for our intention to duplicate rat experiments with human experiments. Theoretically, we must assume that all fundamental principles of behavior are the same in organisms with a similar type of nervous system, such as rats, cats, dogs, chimpanzees, and children. It does not follow, however, that a certain principle must therefore manifest itself in the same way in each one of these animals. Since generalizations from lower mammals to human beings, though natural and desirable, are beset with great hazards, it would seem imperative to determine experimentally how learning efficiency in human subjects is influenced by factors whose function in white rats has already been demonstrated. Since the results of our rat experiments have led us to formulate the general principle that any condition at the point of choice which tends to delay the subject will accelerate learning (**5**), we must assume its presence in human behavior too. If it could not be observed in human learning we might assume either that our principle is incorrectly formulated, or that masking factors make its actual manifestation different in the two types of subjects.

The Experiment

The subjects were 150 college students, mostly sophomores. All were given the same preliminary training, on the basis of which they were divided into six groups matched roughly as to maze learning ability.

The maze used in the preliminary training was of the spoke wheel type with buttons arranged as shown in Fig. 1. A subject was told that one of the five buttons in each spoke was the correct one and would ring a bell when touched with the stylus. His aim was to learn to go around the circuit three times without making an error.

As soon as a subject had learned the preliminary maze he was assigned to one of six groups in such a way that the distributions of scores in terms of errors and trials were roughly equal for the six groups. Since it was our aim to find for any one particular subject five others with similar scores quite a few more subjects were tested in the preliminary maze than were finally selected for the actual experiment.

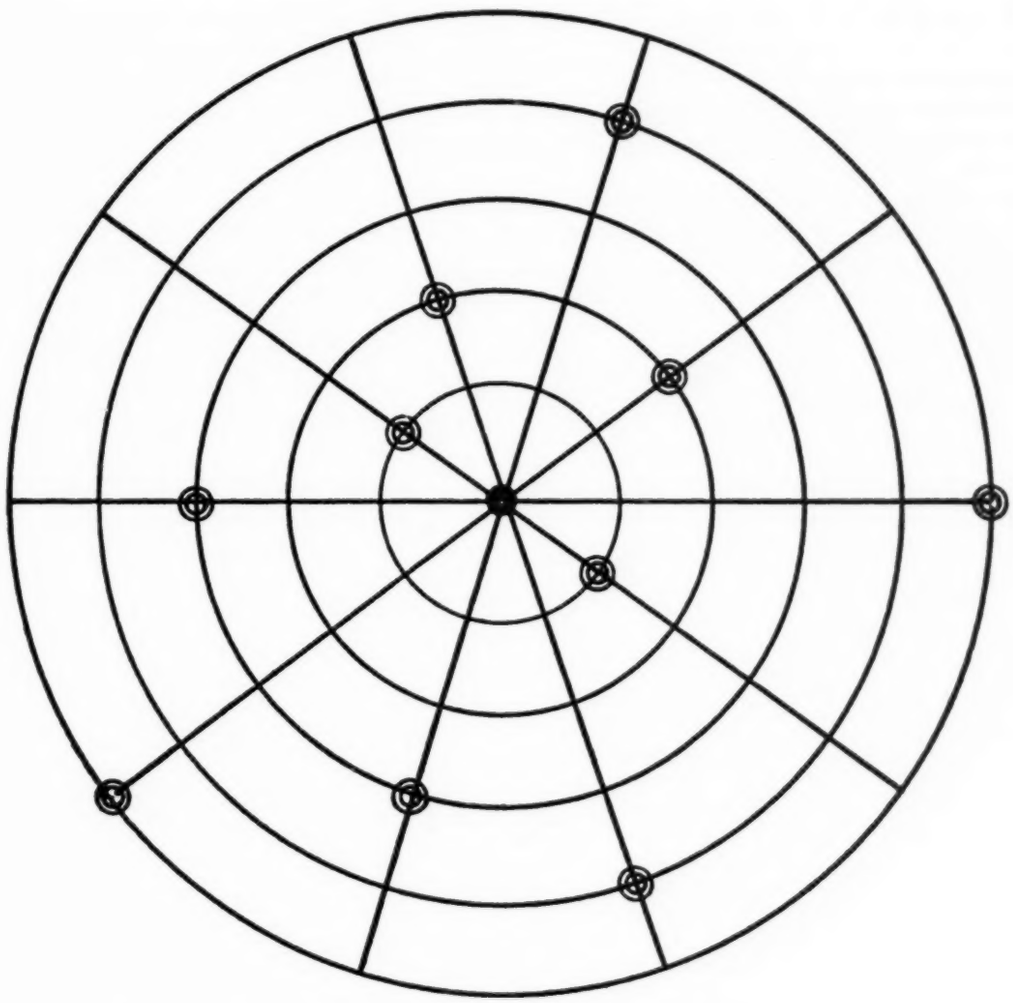

FIG. 1. The preliminary maze (4 in. diam.). The correct points of contact are double-encircled.

Our success in matching is indicated in Table I. The average measures for the six distributions are fairly similar. The poorest group in terms of errors is the obstacle group which, as noted later, has the best learning score in the actual experiment. The correlation coefficients show that in four of the six groups there was a fair tendency for the rankings in the actual experiment to

TABLE I

SCORES IN THE PRELIMINARY MAZE

Groups	*N*	Errors		Trials		Correlation (Rank Difference) of Trials in Preliminary and Actual Experiments
		Ave.	A.D.	Ave.	A.D.	
I. Control	25	73.3	25.7	13.7	5.1	.55
II. Obstacle	25	81.9	32.4	13.4	4.4	.58
III. Delay after choice	25	75.7	30.3	12.9	4.0	.57
IV. Delay at choice	25	73.3	22.0	12.4	2.3	.32
V. Shock-wrong	25	73.2	28.5	13.8	3.6	.25
VI. Shock-right	25	80.3	31.1	12.8	3.8	.65

Note: The conditions were the same for all groups in the preliminary maze. The names of the groups indicate the conditions in the actual experiment with the punchboard maze.

be similar to those in the preliminary maze. We realize the crudeness of our method of matching, especially in view of what is known about the reliability and validity of maze scores. We might be able to speak with greater certainty about the comparability of the groups if we had had a second preliminary test. However, since the measures in the experimental maze (Tables II and III) are similar for groups I, V and VI as expected we might assume that we were fairly successful in matching all the groups.

The apparatus for the actual experiment was a punchboard maze with 30 pairs of holes arranged as in Fig. 2. The subjects were instructed to learn the maze by inserting a stylus in the

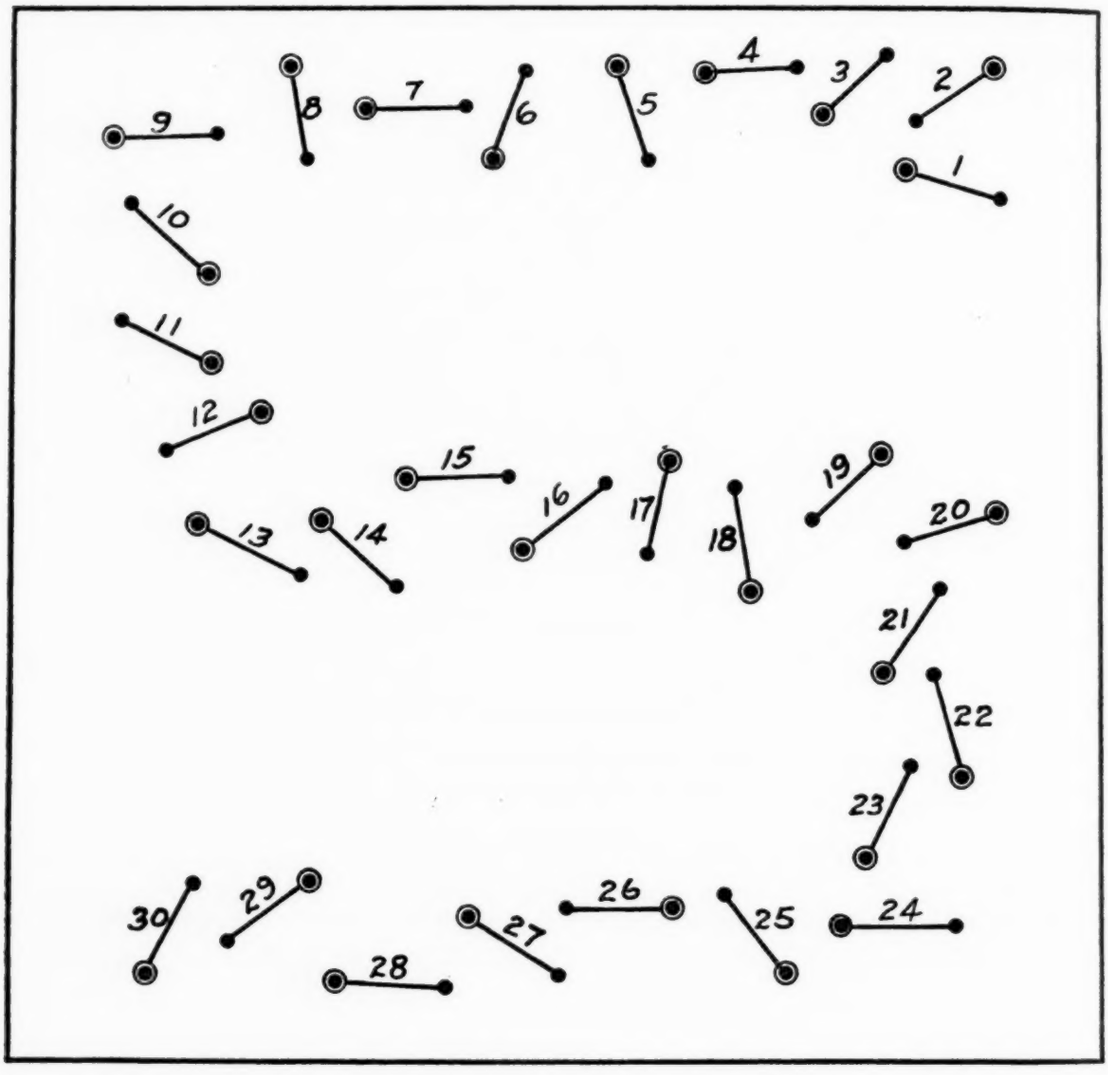

FIG. 2. The punchboard maze (10 in. square). The correct points of contact are marked in this diagram by ⊙.

correct series of 30 holes. In any pair of holes the insertion of the stylus into the correct hole caused a bell to ring. If the wrong hole was punched, as indicated by the absence of the bell signal, the mistake had to be corrected before the subject could go on to the next pair of holes. The criterion of learning was three successive errorless trials of the whole maze.

The conditions for the six groups varied as follows:

Group I, *control*. A smooth stylus was used, slightly smaller than the holes, so that it could be inserted without difficulty.

Group II, *obstacle*. A threaded stylus was used that had to be screwed into each hole. Five to six complete turns were needed until the stylus touched the bottom of the hole. This operation took about 5 sec. per hole.

Group III, *delay after choice*. Here again a smooth stylus was used as in group I, but the subjects were required at each pair of holes to pause for three seconds with the stylus poised over

the hole they had chosen. If this turned out to be the wrong hole they could go to the right one without further delay.

Group IV, *delay before choice.* The conditions were the same as in the preceding group, except that the subjects were delayed with the stylus poised midway between the two holes. (This group may be misnamed, because we have no assurance that a subject might not, contrary to instructions, make a choice during the delay period.)

Group V, *shock for wrong choice.* The smooth stylus was used. The left hand rested on a board with two mercury cups for the index and middle fingers. Punching the wrong hole with the right hand produced a single electric shock to two fingers of the other hand. The current had a strength of 1.0 milliamperes and was controlled by a vacuum tube, as in our previous experiments.

Group VI, *shock for right choice.* The conditions were the same as for the preceding group, except that the subjects were receiving a shock for every correct response.

The criterion of learning was three successive errorless trials.

Results

The results are given in terms of the average numbers of errors and trials in Tables II and III.

TABLE II

Errors in the Punchboard Maze

Groups	Ave. No. of Errors	σ	Critical Ratios = Diff./$\sigma_{Diff.}$				
			II	III	IV	V	VI
I. Control.....	82.1	40.1	4.2	2.3	0.5	0.1	0.1
II. Obstacle.....	43.4	22.0		2.8	5.8	4.2	4.6
III. Delay-after...	61.1	22.4			2.8	2.0	2.4
IV. Delay-before..	78.0	19.9				0.3	0.4
V. Shock-wrong..	81.2	39.0					0.1
VI. Shock-right...	80.8	34.2					

TABLE III

Trials in the Punchboard Maze

Groups	Ave. No. of Trials	σ	Critical Ratios = Diff./$\sigma_{Diff.}$				
			II	III	IV	V	VI
I. Control.....	15.9	7.6	4.2	2.9	0.8	0.7	1.2
II. Obstacle.....	8.6	3.8		2.6	7.1	5.0	3.5
III. Delay-after...	11.1	2.9			3.4	3.7	1.9
IV. Delay-before..	14.4	4.0				1.7	0.6
V. Shock-wrong..	17.5	8.0					2.0
VI. Shock-right...	13.6	5.9					

Our most notable finding is that the obstacle group learned the maze in about half the number of trials and with about half the number of errors as the control group. The critical ratios for the differences between the averages of the two groups (errors 4.2, trials 4.2) definitely indicate the facilitating effect of an obstacle after the point of choice in human learning.

The two shock groups were in no way different from the control group.

The delay groups differed from each other. The delay-before-choice group was similar to the control group, while the delay-after-choice group was about midway in efficiency between the control and the obstacle groups. However, since the critical ratios of the differences between the averages of the control and the delay-after-choice groups are below 3 for both measures, we can only suggest that our results indicate, but do not demonstrate, that a delay of this type may have an accelerating effect.

Discussion

1. Comparing the results of this experiment with those of our animal experiments we notice in the first place a striking agreement as to *the facilitating effect of an obstacle after the point of choice whether we use animal or human subjects*. The nature of the two obstacles in the two situations was different, to be sure. In the case of the rats gaps in the floors of the two choice alleys made the animals hesitate at the point of choice (**8**), while in the case of the human subjects the turning of the stylus lengthened the interval between the act of choice and the experience of success or failure.

2. In the second place, there is some indication of agreement between the results of the animal and human experiments on the accelerating effect of a delay after choice. The two situations were almost identical. The rats were restrained for a few seconds at the entrance of the alley which they had chosen (**9**), while the human subjects were delayed at the hole which they had chosen.

It would seem plausible to consider the obstacle and delay-after-choice situations as fundamentally similar and to assume that the facilitating effect of the obstacle might be due to the delay which it induced. We are impressed, however, by Honzik and Tolman's argument (**3**) that it is not the delay as such that facilitates learning, but most likely a temporary increase in the subject's attentivity induced by the obstacles causing the delay.

The fact that the delay-after-choice situation was not as effective as the obstacle-after-choice situation may have been due to the particular conditions of our experiment. A slight increase or decrease in the enforced pause might have made a considerable difference in learning efficiency.

3. In the third place, the results of the animal and human experiments disagree as to the facilitating function of electric shock after the point of choice. In rats, as in other sub-human subjects, shock after the point of choice invariably accelerates learning (**6**). In the

present as well as in a previous experiment with human subjects (7) shock has no such effect. We do not claim that shock never has a facilitating effect in human learning. Indeed, other experimenters have claimed the existence of such an effect (1, 2). All we can say is that again we have been unable to observe it.

The significant aspect of the discrepancy between the human and animal experiments with shock is that in rats shock after choice induces a pause at the choice point while in human subjects it does not. If our conclusions about the effects of a pause are correct we should expect such a discrepancy.

4. Finally, there remains the situation in which the subjects of group IV were delayed midway between the two holes presumably before they had chosen one of them. The absence of any accelerating effect is interesting in comparison with the probable existence of such an effect when the subjects were restrained before they inserted the stylus into the hole which they had chosen, as in group III. If we could assume that the choice actually did not take place until the subjects were given the signal 'go,' we might regard the absence of the facilitating effect in the delay-before-choice situation as another indication that an accelerating factor must occur after or during the act of choice if it is to be effective. Unfortunately we have no evidence that our subjects did not choose before the 'go' signal.

Summary

Six groups of college students, roughly matched as to maze learning ability, learned a punchboard maze under various conditions. Comparisons with the control group permit the following conclusions:

1. Learning was definitely facilitated when an obstacle was placed between the point of choice and the goal.

2. Delay after the point of choice facilitated learning to some extent, while delay before choice did not do so at all.

3. Electric shock, whether given for correct or incorrect responses, had no facilitating effect.

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References

1. Bunch, M. E., Certain effects of electric shock in learning a stylus maze, *J. comp. Psychol.*, 1935, **20**, 211–242.
2. Crafts, L. W., and Gilbert, R. W., The effect of punishment during learning upon retention, *J. exp. Psychol.*, 1934, **17**, 73–84.
3. Honzik, C. H., and Tolman, E. C., The action of punishment in accelerating learning, *J. comp. Psychol.*, 1938, **26**, 187–200.
4. Lashley, K. S., The mechanism of vision: I. A method for rapid analysis of pattern-vision in the rat, *J. genet. Psychol.*, 1930, **37**, 453–460.

5. MUENZINGER, K. F., The law of effect, *Psychol., Rev.*, 1938, **45**, 215–218.
6. MUENZINGER, K. F., Motivation in learning: I. Electric shock for correct response in the visual discrimination habit, *J. comp. Psychol.*, 1934, **17**, 267–277.
7. MUENZINGER, K. F., Motivation in learning: II. The function of electric shock for right and wrong responses in human subjects, *J. exp. Psychol.*, 1934, **17**, 439–448.
8. MUENZINGER, K. F., AND NEWCOMB, H., Motivation in learning: V. The relative effectiveness of jumping a gap and crossing an electric grid in a visual discrimination habit, *J. comp. Psychol.*, 1936, **21**, 95–104.
9. MUENZINGER, K. F., AND FLETCHER, F. M., Motivation in learning: VII. The effect of an enforced delay at the point of choice in the visual discrimination habit, *J. comp. Psychol.*, 1937, **23**, 383–392.
10. WRIGHT, H. F., The influence of barriers upon strength of motivation, *Contr. psychol. Theor.*, 1937, **1**, No. 3.

CHANGE IN SIZE OF THE AFTER-IMAGE INDUCED IN TOTAL DARKNESS

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It is now an accepted fact that convergence and accommodation influence the visually perceived size of objects. Swenson (**2**) and Hermans (**1**) have independently shown that if the retinal image of an object is held constant, both convergence and accommodation for a point nearer than the object cause the percept to diminish in size, whereas relaxed accommodation and convergence are correlated with an apparent enlargement of the object.

The sensory processes underlying this effect are complex. When the eyes converge or accommodate under normal conditions, it is probable that stimulation occurs in at least two sense modalities. In the first place, the movement of the eyes in their sockets and the change in curvature of the lens probably set up kinesthetic or tactile impulses in the appropriate afferent nerves. Furthermore, under ordinary conditions, both convergence and accommodation change the pattern of visual stimuli which are affecting the retinae. When convergence is changed, the shifting pattern of visual stimulation is perceived as a drawing-together or further separation of the double images of objects in the visual field. On the other hand, when accommodation is altered, the outlines of certain objects appear to become more blurred, while the definition of other objects increases.

Since, under normal conditions, convergent and accommodative movements of the eyes produce both knesthetic and visual stimulation, it has been impossible to determine whether the size change resulting from convergence and accommodation is due to the kinesthetic factor or to the visual factor or to both. It has frequently been assumed that the kinesthetic element was responsible for the size effect, but no proof of this assumption has been presented. It is believed that the observations about to be described lend some support to this kinesthetic hypothesis.

I. The Size Effects

A. If, after a few minutes of dark adaptation, a white card is briefly illuminated with strong light, a positive after-image of the Swindle's ghost variety will gradually appear in the ensuing darkness. The effect is best obtained when a 100-watt bulb, shielded from the eyes and located about two feet above the card, is flashed on for approximately .5 sec.

By chance, while observing these images in *complete darkness*, it was noticed that if, while attending to the image, the head was moved backward, the image appeared to enlarge markedly, whereas it appeared to shrink when the head was moved forward. Fourteen observers unanimously confirmed these observations. This effect was in the same direction as the size change of a negative after-image observed in daylight which results from changing the distance between the eyes and the projection background. Stating this in a different way, it may be said that the size effect was in the direction predicted by Emmert's law.

B. A size effect similar to the one just described was observed under the following conditions, by a majority of the subjects. The white card was held in the hand and a Swindle's ghost image of the card was obtained. As soon as the image became clear, the observer moved the card *in the dark* toward his eyes. He was instructed to hold his head still and to 'look at' the image as though it were an object being viewed in the light. Under these conditions eleven subjects observed a shrinkage in the after-image. This effect was not as compelling as the size change due to head movement and usually appeared only after several attempts. Three subjects never observed the phenomenon.

Since both of these effects were observed in a totally dark room, they furnish clear demonstrations of the efficacy of non-visual factors in the visual perception of size. It was thought that these size phenomena were probably a function of the kinesthetic component of convergence or of accommodation. It seemed reasonable to suppose that in the case of size effect *A*, when the head was moved backward while viewing the after-image, the eyes diverged slightly and relaxed accommodation in such a way that they remained fixated on approximately the same spot, even though they were being guided by no fixation point. Conversely, it was believed that the eyes tended to converge and to increase their accommodation when the head was moved forward. In other words it was felt that the eyes might have been behaving in the dark in a similar fashion to the way they are known to act when the head is moved backward and forward while fixating an object in the light.

In similar fashion size effect *B* might well be dependent upon increased convergence and accommodation resulting from the 'knowledge' gained through the proprioceptors, that the card was approaching the eyes.

To test the hypothesis that convergence and accommodation are basic to the phenomena, an experimental analysis of size effect *A* was undertaken.

II. Experimental Analysis

1. The first experiment consisted of having the subject obtain a Swindle's ghost after-image and observe it while the head was moved forward and backward with the eyes continuously fixated on a point of light which moved with the head and which prevented any changes in accommodation or convergence. It was assumed that if no size change in the after-image was noticed under these conditions it would show that the size effect in the dark was due to eye movement and that it was not a direct result of the contraction of the muscles of the back or neck or of labyrinthine stimulation.

Twelve observers were used in this test. Eight of these subjects reported that the after-image remained constant in size. Three subjects reported a slight enlargement of the after-image as the head was moved back but this enlargement was said to be much less pronounced than when the head was moved during observation of the image in complete darkness. One observer stated that the after-image enlarged markedly under the experimental conditions, but this subject also reported a tendency for the fixation light to double, showing that fixation was not always being maintained.

With one possible exception, then, all of the observers reported that the size effect was completely absent or markedly reduced when convergent and accommodative changes were ruled out. This suggests rather strongly that kinesthetic-tactile stimulation resulting from convergence and accommodation lies at the basis of the size phenomenon.

2. This conclusion is strengthened by a second series of observations made with the use of the apparatus diagrammed in the upper part of Fig. 1. This diagram represents the side view of the apparatus. In the center of the white card, which served as the source of the after-image, was a small hole which could be illuminated from the rear by red light. A sheet of transparent glass was set at such an angle that a spot of green light originating in a lamp set above the glass was perceived by an observer whose head was in 'eye position 2' as originating at the point behind the glass labelled 'apparent green.' Headrests (not shown in the figure) were so arranged as to allow the observer's head to move forward and backward one foot. 'Eye position 1' represents the position of the observer's eyes when leaning forward against the front headrest and 'eye position 2' indicates the position of the subject's eyes when the head was against the back headrest. When the head was held forward the red spot of light was two feet from the eyes; when the head was in 'eye position 2' the apparent source of the green spot was two feet from the eyes, whereas the red spot was three feet distant.

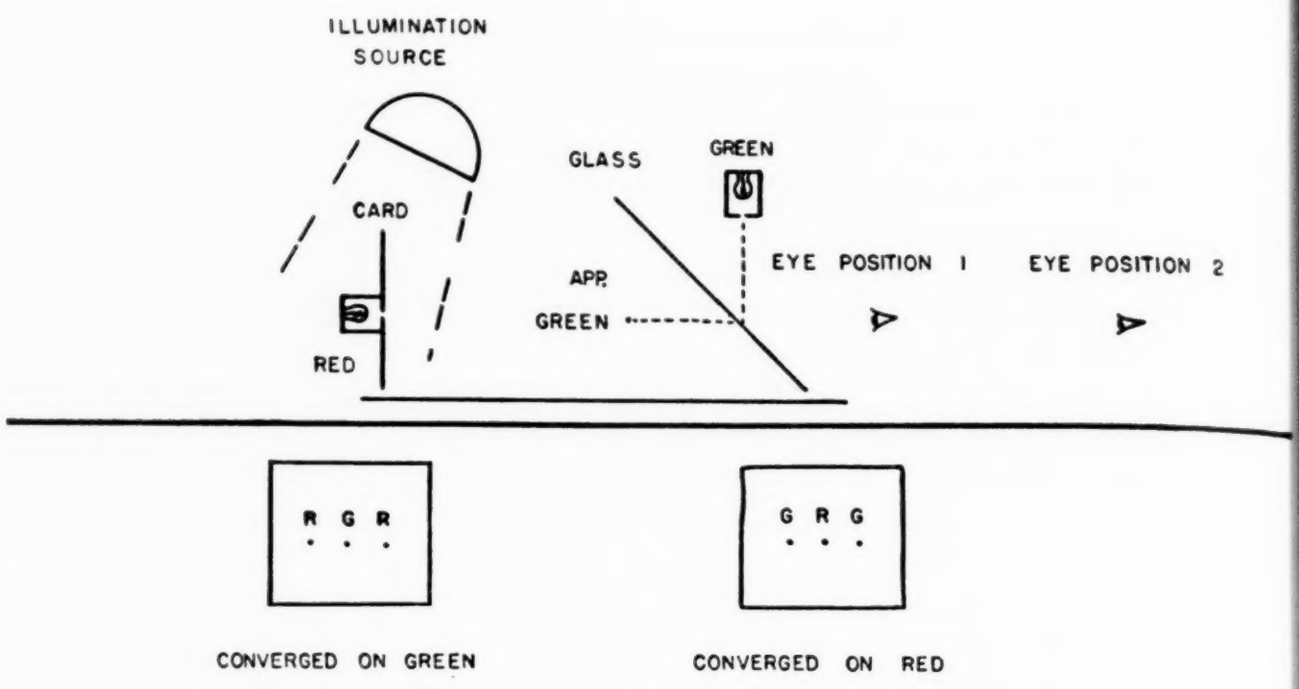

FIG. 1. Side view of apparatus.

If, in a dark room, an observer in 'eye position 2' fixated the green spot, it appeared as a single spot of light, while the red spot (since it was located one foot behind the fixation plane) was perceived as double. The appearance of the light spots is represented in the lower left corner of Fig. 1. If the observer's eyes were converged on the red fixation point, one red dot and two green dots were seen. (Figure 1, lower right corner.) By obtaining a report from the observer as to which of the colored spots was seen as double, it was possible to infer his fixation distance.

The procedure was as follows: With the observer's head in 'eye position 1,' all lights, with the exception of the red dot, were extinguished. The subject was instructed to fixate this point of light, and as he was doing so, the card was briefly illuminated. The red dot was turned off simultaneously with the cessation of the light from the illumination source. When the after-image appeared, the subject moved his head in total darkness to 'eye position 2' and attended to the size effect. As soon as his head was in this position, both the red and the green dots were flashed on for approximately one second. During this one second interval, the subject observed the spots and reported on them as soon as the test was over.

If, when the observer moved his head from 'eye position 1' to 'eye position 2,' he had maintained a steady convergence (*i.e.*, his eyes had remained immobile in their sockets), he would be expected to report one green and two red spots, but if he had diverged as his head moved back in such a way as to maintain his original fixation, a report of two green and one red would be anticipated.

Of the thirteen observers employed, twelve saw the red spot as single and the green spot as double. This indicates that these observers were diverging as they moved their heads back, and this in turn supports the hypothesis that convergence or accommodation lies at the basis of the size effect.

It is difficult to account for the observations of the one exceptional subject. Though the size effect was observed consistently, this subject always reported a doubling of the red spot while the green was seen as single. It is possible that factors other than convergence and accommodation produced the size change in his case. It is also possible, however, that though his eyes diverged as his head moved backwards, they immediately reconverged to the original fixation distance before the light spots were flashed on. This might happen if the subject exhibited a tendency toward convergent strabismus. Unfortunately, however, the presence or absence of this condition could not be determined before the observer left the laboratory.

III. Discussion

It is believed that the size effects described in this report furnish incontrovertible evidence in favor of the view that visually perceived size may be influenced by certain non-visual factors. Furthermore, the experimental analysis has shown that convergence or convergence and accommodation are, in some way, related to these size phenomena, and it seems likely that this relationship is a direct one. The kinesthetic and tactile stimuli from the neck, back and labyrinth in the case of size effect *A* and from the arm in size effect *B* are undoubtedly necessary for these effects, but it appears that they are hardly sufficient. The fact that the change in size of the after-image during head movement is inhibited when changes in accommodation and convergence are ruled out suggests that the tactile-kinesthetic stimuli from these eye movements are basically important, whereas the other kinesthetic cues have little or no *direct* effect upon the visual perception of size. Rather, it would seem, the proprioceptive impulses from the moving head or arm serve chiefly to induce the convergent and accommodative changes, either reflexly, as is likely in the case of the first size phenomenon, or through setting off a previously conditioned response.

It is, of course, possible that in seeking to control convergence and accommodation by means of a fixation point which moved with the head, certain factors were introduced which would render the results liable to misinterpretation. Furthermore, the fact that there was not complete agreement among the observers in either of the two experiments would suggest caution in drawing conclusions. For

these reasons the possibility that kinesthetic stimuli from the neck, back, labyrinth and arm play more than an indirect role in the size phenomena still must be left open.

Yet, in spite of the fact that the part played by kinesthesis from the body cannot be described completely, nevertheless, it can be stated with some confidence that tactile-kinesthetic stimuli from the moving eyes can influence directly the visually perceived size of objects in the absence of any changing visual cues.

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References

1. Hermans, T. G., Visual size constancy as a function of convergence, *J. exp. Psychol.*, 1937, **21**, 145–161.
2. Swenson, H. A., The relative influence of accommodation and convergence in the judgment of distance, *J. gen. Psychol.*, 1932, **7**, 360–380.

A NOTE CONCERNING HARTMANN'S STUDIES OF INTERSENSORY EFFECTS

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Hartmann has published several studies dealing with the effects of auxiliary stimuli upon sensory thresholds. The first of these investigations (**3**) has been appraised by Honzik (**6**) who computed critical ratios from the data given by Hartmann. These critical ratios ranged from .43 to 14.24. On a basis of generally high critical ratios (13 out of 19 were of such a magnitude as to rule out chance) Honzik concluded "that with light in the left eye, the visual acuity of the right eye was truly increased."

With reference to a second group of experiments (**4**), in which Hartmann reported an effect upon visual acuity of the simultaneous excitation of other sense modalities, Honzik stated, without determining critical ratios, that "inspection of the means and A. D.'s (as given by Hartmann) indicates that the critical ratios would probably be of the same magnitude." On the basis of certain facts in physiological optics, Honzik expressed the opinion that Hartmann's experiments seemed to be only mildly confirmatory, rather than demonstrative (as Hartmann had stated) of a general diffusion of effects in the cerebral cortex.

Although Hartmann's data are impressive because of the consistency with which 'facilitation' appears, they nevertheless raise certain doubts when considered in the light of their statistical significance.

With a revival of interest in the field of intersensory effects (**2**), (**8**), a field which still presents considerable equivocal material, it becomes doubly important to clarify as much as possible the work which has been done in the past.

The present writer has subjected Hartmann's data to statistical treatment and reports the following critical ratios which may be utilized to supplement Hartmann's report (**4**).

Critical ratios for comparisons between the experimental condition in which there was an auxiliary stimulus and the normal condition are presented here for the data given in Hartmann's report (**4**). Each critical ratio is preceded by the subject's initials as they were listed by Hartmann. The different conditions of Hartmann's experiment are listed here in outline form.

Hartmann's Table I:

(*a*) Auxiliary auditory stimulus of 2100 dv., method of limits used for measurement of visual acuity, black squares on a white background. Critical ratios are as follows: Ad, 1.29; Mac, 1.13; Cal, 2.50; Kos, .57; Ba, 1.18; Kr, 2.48; Wi, 2.24; Ma, 1.18; Eln, 2.86; Gwh, 4.26.

(*b*) Same as (*a*) but with white squares on a black background. Critical ratios are: Ad, 1.03; Mac, 1.24; Cal, 2.26; Kos, .24; Ba, 1.54; Kr, 2.33; Wi, 2.05; Ma, .78; Eln, 2.50; Gwh, 4.06.

(*c*) Same as (*a*) but with auxiliary auditory stimulus of 180 dv. Critical ratios are: Wol, 2.42; Me, 1.59; Jur, 1.05; For, 1.46; Sch, 1.59; Sch, 1.37; Nes, 2.17; Ber, 1.16; Eln, 2.95; Gwh, 3.21.

(*d*) Same as (*b*) but with auxiliary auditory stimulus of 180 dv. Critical ratios are: Wol, 3.21; Me, 1.77; Jur. 2.62; For, .67; Sch, 1.07; Sch, 2.23; Nes, .94; Ber, .76; Eln, 2.45; Gwh, 4.45.

Hartmann's Table II:

(*a*) Auxiliary pleasant olfactory stimulus, black squares on white background. Critical ratios are: Fr, .65; Sc, 3.61; Vs, 3.37; No, 4.53; Ca, 2.69.

(*b*) Same as (*a*) but white on black. Critical ratios are: Fr, .75; Vs, 1.64; No, 4.60; Ca, 1.48.

(*c*) Same as (*a*) but unpleasant odor. Critical ratios are: Mo, 6.21; Dr, 2.64; Ma, 2.00.

(*d*) Same as (*b*) but unpleasant odor. Critical ratios are: Mo, 2.78; Dr, 1.99; Ma, 2.24.

Hartmann's Table III:

(*a*) Auxiliary tactual stimulus, black on white. Critical ratios are: Vr, .54; Ca, .42.

(*b*) Same as (*a*) but white on black. Critical ratios are: Vr, .80; Ca, 1.09.

Hartmann's Table IV:

(*a*) Auxiliary pain stimulus, black on white. Critical ratios are: Le, 1.11; Ge, 1.50.

(*b*) Same as (*a*) but white on black. Critical ratios are: Le, 2.78; Ge, 1.23.

Since it was stated by Hartmann that he used the same procedure in these experiments (**4**) as in his first experiment (**3**) where he used

an auxiliary visual stimulus, it was assumed that the means which he reported represent 20 judgments (10 ascending and 10 descending). Hence the formula used for determining the standard error of the mean was: $\sigma_M = \frac{\sigma}{\sqrt{N-1}}$ (1). Hartmann's average deviations were converted to standard deviations by the formula: $\sigma = 1.2533$ AD. (1).

Critical ratios were also determined for Table V from Hartmann (4). In this table he presented data to check, by the method of constant stimuli, the results obtained by the method of limits (see conditions (*a*) and (*b*) under Hartmann's Table I above). Hartmann determined visual acuity with a simultaneous auxiliary tone of 2100 dv. and then compared the results with visual acuity measurements made when there was no sound stimulation. The critical ratios computed by this wrtier for the average of Hartmann's six subjects are: For black on white, 1.20, and for white on black, .56. The formula used for determining the standard error of the mean in these latter two cases was: $\sigma_M = \frac{\sigma}{\sqrt{N-3}}$ (1), since the frequency was less than 10. It was impossible to obtain individual critical ratios for each subject since measures of variability were not reported by Hartmann for each subject. He reported only the average deviation for the mean of the total of all subjects under each condition.

In another study designed to demonstrate the effects of different intensities of light upon thresholds of pitch and auditory intensity Hartmann (5) reported critical ratios ranging from .625 to 5.114. Using Hartmann's standard deviations, the present writer has recomputed the critical ratios and obtained different values. For a comparison of the pitch limen at high and low levels of illumination (Procedure *A* in Hartmann's Table I, (5)), the critical ratio becomes .99, and for intensity, 1.24. For Hartmann's Procedure B the critical ratio for intensity becomes 1.89. For Procedure C, the critical ratio for the pitch limen turns out to be .41, and for the intensity limen the critical ratio is .38.

For Procedure A, since N equals 20, the formula used by the present writer to determine the standard error of the means was: $\sigma_M = \frac{\sigma}{\sqrt{N-1}}$ (1). For Procedures B and C, since N equals 10, the formula used here was: $\sigma_M = \frac{\sigma}{\sqrt{N-2}}$ (1). Since Hartmann reported correlations, the following formula was used by this writer to determine the standard error of the differences: $\sigma_D = \sqrt{\sigma_{M_1}^2 + \sigma_{M_2}^2 - r_{12}\sigma_{M_1}\sigma_{M_2}}$ (1). The present writer is not convinced that Hartmann was justified

in correlating his data; if he was not, the critical ratios given above would be even less than they are.

Unequivocal evidence, either for or against intersensory facilitation, is lacking if the critical ratios reported here for the experiment on visual acuity are regarded as criteria. There are 35 C. R.'s less than 2.00, 20 C. R.'s between 2.00 and 2.99, and 10 C. R.'s above 3,00. Four of the C. R.'s above 3.00 were obtained for Hartmann himself acting as subject. In the olfactory part of the experiment alone, there were 5 C. R.'s above 3.00 and 5 C. R.'s between 2.00 and 2.99 (out of a total of 15 C. R.'s for that part of the experiment). These critical ratios, *i.e.*, four for Hartmann and ten from the olfactory part of the experiment, account for a great proportion of the presumably significant C. R.'s. Significance depends upon whether the criterion is placed at 2.00 or 3.00 (see McNemar (7) pp. 336–7). The criterion is, of course, usually placed at 3.00. From a study of Hartmann's method in the olfactory part of the experiment, it might be concluded that factors of attention or suggestion were responsible for the reported variation. It must be said, however, that there was a consistent difference in the same direction between means in all of Hartmann's subjects. This fact may be indicative of some small intersensory effect.

In the case of the experiment on auditory limens, it is not known what methods were employed by Hartmann to obtain the critical ratios he reported. By the use of the above generally accepted formulæ the low critical ratios which this writer obtained would tend to minimize any conclusions about intersensory effects.

This writer has duplicated Hartmann's conditions for visual acuity. Using an auxiliary stimulus of 770 dv. at a supraliminal intensity of 60 db critical ratios were obtained which were all fractional. These results will be reported more extensively later.

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References

1. Dunlap, J. W., and Kurtz, A. K., *Handbook of Stastical Nomographs, Tables and Formulas*, 1932, New York: World Book Co., vii + 163.
2. Gilbert, G. M., Intersensory facilitation and inhibition, *J. gen. Psychol.*, 1941, **24**, 381–407.
3. Hartmann, G. W., I. The increase of visual acuity in one eye through illumination of the other, *J. exp. Psychol.*, 1933, **16**, 383–392.
4. Hartmann, G. W., II. Changes in visual acuity through simultaneous stimulation of other sense organs, *J. exp. Psychol.*, 1933, **16**, 393–407.
5. Hartmann, G. W., The facilitating effect of strong general illumination upon the discrimination of pitch and intensity differences, *J. exp. Psychol.*, 1934, **17**, 813–822.
6. Honzik, C. H., A note on Hartmann's experiments showing the effect on visual acuity of simultaneous stimulation of other sense organs, *J. exp. Psychol.*, 1933, **16**, 875–878.
7. McNemar, Q., Sampling in psychological research, *Psychol. Bull.*, 1940, **37**, 331–365.
8. Ryan, T. A., Interrelations of the sensory systems in perception, *Psychol. Bull.*, 1940, 37, 659–698.

PERCEPTUAL FLUCTUATIONS OF ILLUSIONS AS A POSSIBLE PHYSICAL FATIGUE INDEX*

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Hollingworth (**2**) has suggested that perceptual fluctuations may be used as a fatigue index. His work supports the findings of Ash (**1**) and he reaches the conclusion that an increase in the rate of fluctuations of illusions at reversible perspective shows an index of fatigue. However, in his experiment, he used four adult subjects, working four days—three hours in the forenoon and three hours in the afternoon. The exact type of this work is not definitely stated and ocular fatigue might be as much a causal factor as physical fatigue.

The present study is different from that of Hollingworth inasmuch as it uses:

1. Four types of fatigue stituations and a control group.
2. Shorter duration of time to obtain physical fatigue and that fatigue of a type having a relatively small amount of ocular fatigue.
3. More subjects.
4. Different illusions on the same group of subjects to determine if some illusions would reveal a better fatigue index than others.
5. An illusion that has three ways of changing and compares it to one that has two ways.

Procedure

The illusions used were black figures on a white background. The four figures used were the open cube illusion, Fig. 99, Woodworth (**4**); the ambiguous stair, Fig. 100, *ibid;* the cross, Fig. 97, *ibid;* the three cube figure, Fig. 101, *ibid.*

The last mentioned figure had three possibilities of being observed: (1) two cubes on top one on the bottom; (2) one cube on top and two on the bottom; or (3) a six pointed star. The others had but two possible fluctuations. All of the figures were such as to be plainly visible at twenty feet and were observed at that distance for one minute.

The subjects were male college students, in all of the fatigue situations. In the control group, two classes in psychology were tested at the beginning and end of the period. The female subjects were tabulated separately, but as there appeared to be no sex difference, the two groups were put together. This group was tested with no fatigue situation intervening. The same material was used to see if a learning factor might be operating. In all cases the subjects were told to count the number of fluctuations that occurred while looking at the figures.

The fatigue situations were football practice, basketball practice, rugby practice, and a general fatigue exercise. The general fatigue exercise was perhaps more severe than the others especially if considered from the standpoint of a quick oxygen debt. The task was one of pulling

* This work was done in cooperation with the Physical Education Department of Purdue University, Lafayetre, Indiana.

a machine which created a 50 pound pull. The subjects averaged .36 horse power in pulling the apparatus from 400 to 800 feet. Most of the subjects pulled 500 feet. All of the tests except those in rugby were administered individually. In rugby, 28 individuals were selected from a group of 47. This selection was made by taking only those who indicated an increase in fatigue on the recording sheet after the practice period.

The measurement of fatigue was determined on a seven point continuum which ranged from 'very tired' (as tired as you have ever been) at the lower end to 'very fresh' (as fresh as you have ever been) at the other, with four as a neutral point.

Table I shows the fatigue rating before and after the fatigue situation.

TABLE I

FATIGUE RATING OF INDIVIDUALS IN VARIOUS FATIGUE SITUATIONS

Fatigue Situation	N.	Rating *Before* Fatigue	Rating *After* Fatigue	Difference	Time of Test
Football	15	5.1	3.6	1.5	4–6 P.M.
Basketball	20	4.9	2.8	2.1	4–6 P.M.
General Fatigue	45	4.5	2.7	1.8	A.M. and P.M.
Rugby	28	4.8	2.7	2.1	8–10 A.M.

The difference was significant (less than 1 chance in 100) in all cases using Fisher's (3) technique for small samples.

The number of fluctuations per minute before and after the fatigue situation for the open cube illusion can be seen in Table II. Also the

TABLE II

COMPARISON OF FATIGUE SITUATIONS USING OPEN CUBE ILLUSION

Fatigue Situation	Av. Fluc. per Min. *Before* Fatigue Situation	Av. Fluc. per Min. *After* Fatigue Situation	Diff.	No. Reporting Fluc. After Fatigue as			N.
				More	Less	Same	
Football	25.6	26.6	1.0	9	5	1	15
Basketball	19.3	19.3	0.0	8	12	0	20
General Fatigue	19.5	28.8	9.3	25	17	3	45
Rugby	19.2	23.4	4.2	17	9	1	27
Control	19.6	20.1	.5	20	27	2	49

number of persons reporting more fluctuations, less, and the same 'after' fatigue as compared with 'before' is shown in this same table.

The difference for general fatigue and rugby using Fisher's (3) *t* method was less than 1 in 100 that the second series could happen by chance selection. The others were not significant. In the control group, where there was no fatigue, the results were of the type one would expect from random sampling. The average number of fluctuations of the group is slightly more but the number having fewer fluctuations for the second presentation than the first was greater. This information on the control group tends to rule out the possi-

bility of a learning factor which would result in a greater number of fluctuations during the second presentation.

The three cube illusion gave more fluctuations per minute than the two cube illusion in all cases. The results can be seen in Table III.

TABLE III

Comparison of Fatigue Situations Using Three Cube Illusion

Fatigue Situation	Av. Fluc. per Min. *Before* Fatigue Situation	Av. Fluc. per Min. *After* Fatigue Situation	Diff.	No. Reporting Fluc. After Fatigue as			N.
				More	Less	Same	
General Fatigue....	21.4	25.7	4.3	22	10	1	33
Rugby.....	23.5	28.0	4.5	19	7	2	28
Control.....	25.4	26.0	.6	24	21	4	49

The possibility that the means of the rugby group would occur by chance is less than 1 in 100; for the general fatigue group about 2 chances in 100. The negligible difference in the control group and the equal division of persons having more and less fluctuations seem to substantiate the findings in the single cube illusion; namely, that fatigue does effect the fluctuations.

In using different illusions for the same group (rugby) some interesting results were obtained Table IV shows a comparison of 4 illusions.

TABLE IV

Comparison of Different Illusions Using Same Group

Illusion	Av. Fluc. per Min. *Before* Fatigue Situation	Av. Fluc. per Min. *After* Fatigue Situation	Diff.	No. Reporting Fluc. After Fatigue as			N.
				More	Less	Same	
Cube.....	19.2	23.4	4.2	17	9	1	27
Cross.....	13.1	16.5	3.4	17	8	3	28
Stair.....	13.7	15.1	1.4	17	9	2	28
3 Cube.....	23.5	28.0	4.5	19	7	2	28

The differences are significant in all cases except the stair illusion, where the possibility is about one in four that these two means could occur by chance. It will be noticed that some figures appear to fluctuate more rapidly both before and after fatigue than others.

Conclusion

From the evidence obtained it appears that under various types of physical fatigue there is a tendency for individuals to produce more fluctuations after fatigue than before. There is also a possi-

bility that a figure which can be observed in three ways rather than two will have a greater number of fluctuations. It is also apparent that various figures shown to the same group do not have the same possibility for fluctuating. It is possible that the type of fatigue situation may be responsible for differences in fluctuating inasmuch as the basketball and football players, although they have indicated about the same amount of fatigue as the other two groups, nevertheless, do not seem to indicate a definite trend. Because of the fact that in no group or no illusion is there 75 percent agreement, we would conclude that this device would not be adequate as a fatigue index for individual prediction.

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References

1. Ash, I. E., Fatigue and its effects upon control, *Arch. Psychol.*, 1914, **31**, 1–61.
2. Hollingworth, H. L., Perceptual fluctuation as a fatigue index, *J. exp. Psychol.*, 1939, **24**, 511–519.
3. Lindquist, L. E., *Statistical analysis in educational research*, New York: Houghton Mifflin Co., 1940, pp. 48–54.
4. Woodworth, R. S., *Psychology*, New York: Henry Holt & Co., 1934, pp. 433–435.